



# Gradualism in Coordination and Trust Building

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## Gradualism in Coordination and Trust Building

### ABSTRACT

Coordination and cooperation on public projects, as well as trust among society members are important for economic, social and political activities. This dissertation presents essays on the role of gradualism – increasing the stakes of projects slowly over time rather than starting with large-stake projects immediately – in coordination and trust building under various settings. The first two essays are on simultaneous coordination games when there are multiple equilibria in the one-shot game; the third essay is on sequential trust games when the only subgame perfect equilibrium in the one-shot game predicts that no cooperation occurs at all.

The first essay, *One Step at A Time: Does Gradualism Build Coordination?* (joint with Sam Asher, Lorenzo Casaburi, and Plamen Nikolov), uses a multiple-period binary-choice weakest-link coordination experiment and finds that gradualism leads to better coordination in high-stake projects. The findings point to a voluntary mechanism to promote coordination when the capacity to impose sanctions is limited.

This second essay, *Gradualism, Weakest Link and Information: Theory and Coordination Experiments*, extends the first essay and compares the effects of gradualism under various information and payoff structures. It proposes a belief-based learning framework to explore why and when gradualism may help coordination. It compares the role of gradualism in two weakest-link games under two different information structures: a

limited information structure when subjects are only informed whether all group members contribute, and a richer information structure when they are informed exactly how many group members contribute. It finds that richer information feedback facilitates later coordination for the *big-bang* approach when a group is close to success, thus shrinking the advantage of gradualism. Finally, in a third experiment it finds that allowing free riding worsens coordination in all treatments, and gradualism with imperfect monitoring does not perform better.

The third essay, *Does Gradualism Build Trust? Evidence from A Multi-round Experiment*, examines the effect of gradualism in trust building using a multi-round binary-choice trust (investment) experiment. It finds that gradualism leads to higher trustworthiness at the beginning and higher subsequent trust. However, trustworthiness and trust for all treatments sharply decrease in the end; even gradualism cannot avoid this end-of-game effect.

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# 1. ONE STEP AT A TIME: DOES GRADUALISM BUILD COORDINATION?<sup>1</sup>

## 1.1. Introduction

Coordination<sup>2</sup> is important for daily economic, social and political activities (Schelling, 1960; Arrow, 1974). Yet, coordination failure is common (Van Huyck et al., 1990; Cooper et al., 1990; Knez & Camerer, 1994, 2000; Cachon & Camerer, 1996). Such failure can have a large impact on social welfare, as has been explored in multiple subfields of economics and political science: in economic development (Ray, 1998; Bardhan, 2005), in the business cycle (Cooper & John, 1988), in international monetary policy (Krugman & Obstfeld, 2009), and in the establishment of democracy and rule of law (Weingast, 1997).

Although there have been many insightful studies<sup>3</sup> on how to promote successful coordination, a central question remains unsolved: when the capacity to impose sanctions and social pressure is limited, to establish a high level of voluntary coordination, should we choose a *big-bang* approach – starting with the high hard-to-achieve task – or should we

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<sup>1</sup> This is a joint work with Sam Asher, Lorenzo Casaburi and Plamen Nikolov.

<sup>2</sup> In game theory, coordination refers to resolving which equilibrium to play when there are multiple equilibria. In this chapter, we focus on those coordination games with Pareto-ranked equilibria, especially weakest-link (minimum-effort) coordination games.

<sup>3</sup> Some studies find sanction institutions, social pressure and reputation to be mechanisms that promote cooperation (e.g., Olson, 1971; Ostrom et al., 1992; Fehr & Gächter, 2000; Masclet et al., 2003; Gächter & Herrmann, 2010; Bochet et al., 2006; Carpenter, 2007). Others explore methods to facilitate coordination when sanction and social pressure cannot be imposed, such as repetition with fixed group members (Clark & Sefton, 2001), complete information structure (Brandts & Cooper, 2006a), communication (Cooper et al., 1992; Charness, 2000; Weber et al., 2001; Duffy & Feltovich, 2002; Chaudhuri et al., 2009), and between-group competition (Bornstein et al., 2002; Riechmann & Weimann, 2008).

coordinate first on small and easy-to-achieve goals? Does a success with a small goal guarantee an immediate success with a high objective of coordination, or should the objective grow slowly? For example, the European Union (EU) is the result of decades of negotiation.<sup>4</sup> Yet the current economic concerns in the EU bring fresh debates (e.g., Alesina & Giavazzi, 2010; Baldwin et al., 2010) on whether the establishment of the EU and the Euro Zone was a hasty job, given that the standard monetary economics theory has suggested that the level of integration within the EU countries is not high enough to qualify it an optimal currency area and coordination on economic (especially fiscal) policy will be difficult (Krugman & Obstfeld, 2009). This question also applies for domestic reforms which involve substantial levels of coordination, and has been one (although not the only) key issue in the transition from a planned economy to a market economy (Dewatripont & Roland, 1992, 1995; Wei, 1997), and in the democratization process (e.g., Weingast, 1997).<sup>5</sup>

This Chapter aims to address this question and study gradualism within a fixed-size group,<sup>6</sup> a natural and voluntary mechanism to promote coordination where sanction and social pressure are absent. We refer to gradualism as the hypothesis that allowing agents to coordinate first on small and easy-to-achieve goals and increasing the level of goals slowly

---

<sup>4</sup> Increased coordination level and organization growth are two key features in the EU development. The former is the focus of our study; Weber (2006) examines the latter. This history of European Union goes back 60 years to when in the 1950s the European Coal and Steel Community served as the first step towards the development of a European political union, starting with the free-trade arrangement (the Common Market) and leading eventually to the creation of the European Economic and Monetary Union that embodies the euro. A detailed introduction of the EU history can be found at: [http://europa.eu/about-eu/eu-history/index\\_en.htm](http://europa.eu/about-eu/eu-history/index_en.htm).

<sup>5</sup> Of course, economic and political reforms are not just about coordination. A full study of the optimal path of such reforms is beyond the scope of this chapter.

<sup>6</sup> In the real world, group size may also change. Our paper is focused on the case of a fixed-size group or the shorter horizon when the group size has not yet changed. For gradual organizational growth, see Weber (2006). A full comparison of Weber's study and ours can be found in Section 1.2.

facilitates later coordination on otherwise hard-to-achieve outcomes. We can first find its application in intra-organization team building (e.g., new employees are given small initial tasks to help build coordination, which makes sure that they can coordinate well in larger tasks later), inter-organization cooperation and international relations. Examples of international relations include the development of international trade agreements such as the World Trade Organization (WTO),<sup>7</sup> international arms reduction agreements, and regional and international environmental cooperation. In these international agreements, besides a final target (e.g., for arms, tariff or pollution reduction), there is often a series of slowly growing targets in each phase for the international community to examine how countries fulfill the requirements in each phase.

Of course, weakest-link is not always the only payoff structure in these examples (e.g., free riding is sometimes possible.) However, in this chapter we focus on the weakest-link coordination problem, the significance of which has been recognized in the literature aforementioned. A case when free riding is possible is studied in Chapter 2.

To test the gradualism hypothesis, we conduct a computer-based laboratory experiment with repeated interactions which stylizes (but not replicates) a typical coordination setting in the real world. In each period, subjects are endowed with some points (monetary units in the laboratory) and are asked to contribute a given amount (which we call *stake*) to a group project. She can only choose to contribute exactly this amount or nothing to the group project, but not another amount. The path of the stake, which may or may not change over periods, is determined by the treatment she is assigned to. In each period, each member realizes an extra return only when all group members contribute to the project; otherwise

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<sup>7</sup> Like the EU, The development of the WTO involves two aspects: the level of cooperation, and the change in the number of members. This study focuses on the first.

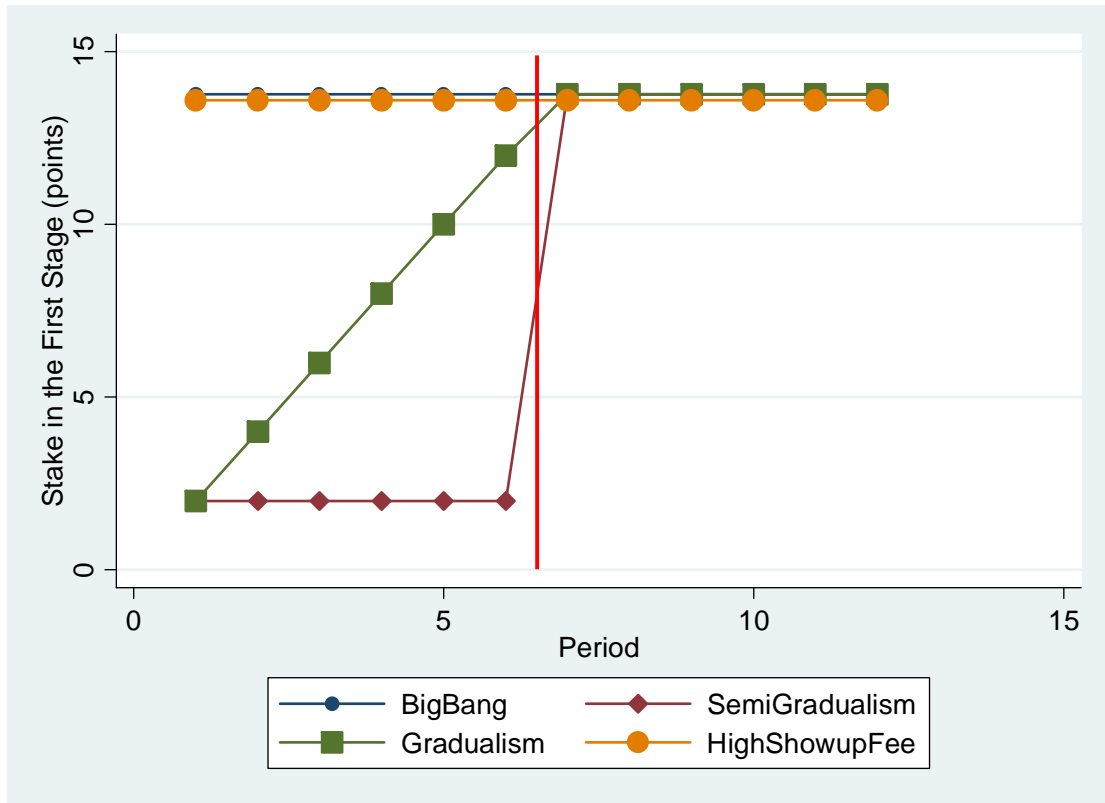
each ends up with only the points that she does not give. This set-up is generally referred to as the minimum-effort or weakest-link coordination games: the payoff depends on one's effort and the minimum effort of group members. Our setting simplifies the payoff function<sup>8</sup> and, specifically, it is a multi-period stag hunt game due to the binary-choice feature in each period.<sup>9</sup>

We introduce three main treatments of stake patterns, which differ in the first six periods but have an identical stake for the final six periods. The first treatment has a constant high stake for all 12 periods; we call this “Big Bang.” The second treatment has a constant low stake for the first six periods and jumps to the high stake for the final six periods; we call this “Semi-gradualism.” Finally, in the “Gradualism” treatment, the stake gradually increases in each of the first six periods until it reaches the high stake in period 7. (See Figure 1.1 for a graphical illustration.) Note that the “Semi-gradualism” treatment falls between the “Big Bang” and “Gradualism” treatments: its stake starts at low but has a sudden increase. Exploiting this design, we test the effect of gradualism on coordination at a high stake.

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<sup>8</sup> The literature of these games generally uses a complex matrix of payoff: there are seven choices of actions, and the payoff depends on own action and the minimum action of others. See Van Huyck et al. (1990), Knez and Camerer (1994, 2000), Cachon and Camerer (1996), Weber (2006), and Chaudhuri et al. (2009).

<sup>9</sup> It is similar to a standard discrete public good game, except that our set-up does not offer an opportunity for any individual to free ride. Free riding is generally considered as a necessary feature of a public good game in the experimental economics literature. However, our set-up also belongs to the “weakest-link public goods game” as described by several theoretical and experimental papers on public goods (e.g., Hirshleifer, 1983; Harrison & Hirshleifer, 1989; Cornes & Hartley, 2007). Cornes and Hartley (2007) provide a general social composition function of public goods with input of individual gifts, which incorporates standard continuous public goods, standard discrete public goods, weak-link (and weakest-link) public goods, good-shot (and best-shot) public goods, etc.



The vertical line between periods 6 and 7 separates the two halves of the first stage; coordination performances of different treatments in the second half (periods 7-12) are the main interests of this study.

Figure 1.1: Stake Patterns of the Treatments in the First Stage

Our experimental results show that the gradualism treatment attains significantly more successful coordination at a high level of stake. In our laboratory experiment, subjects in the “Gradualism” treatment are more likely to contribute in the final high-stake periods than the other two main treatments. In terms of magnitude, the effects are quite large – for example, in the end period, 61.1% of the “Gradualism” groups successfully coordinate, while only 16.7% and 33.3% of “Big Bang” and “Semi-gradualism” groups do, respectively. To establish a successful coordination at a high level, it is better to start at a low level and as importantly, to increase the level slowly.

Our experimental results also suggest an externality of coordination building (or collapse) across different social groups. Those treated in the gradualism setting are about 10 percentage points more likely to cooperate upon entering a new group. However, when they find their cooperation does not get rewarded in a new environment (because the new group members may have been treated differently and have different coordination outcomes in the first stage), they tend to become less cooperative.

To the best of our knowledge, ours is the first study that clearly tests the role of exogenous gradualism in coordination within a given group. It also contributes to the literature on how cooperation evolves over time with varying levels of stakes in multi-period experiments. Our findings point to a simple, voluntary mechanism to promote coordination when the capacity to impose sanctions is limited. It is noteworthy that our game is different from other games with a gradual (or more generally, dynamic) structure. Detailed comparison to the literature can be found in Section 1.2.

The rest of the paper is organized as follows. In Section 1.2, we discuss the related literature. In Section 1.3, we detail the experimental design. In Section 1.4, we present the major results. Section 1.5 concludes.

## **1.2. Literature**

To better understand this study and its contributions, below we compare it with the theoretical and experimental literature about “gradualism” or dynamics in coordination games, prisoners’ dilemma games, and public goods games.<sup>10</sup>

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<sup>10</sup> Gradualism is also studied in sequential-move games like trust (investment) games. Pitchford and Snyder (2004) develop a model where the sequence of gradually smaller investments solves the holdup problem when the buyer’s ability to hold up a seller’s investment is severe. However,

In a laboratory dynamic weakest-link coordination experiment, Weber (2006) studies the dynamics of organizational growth and finds that gradually growing the group size leads to more successful coordination in a large group versus starting with a large group. This study differs from Weber (2006) in three major ways: (1) we explore gradualism in coordination within a given fixed-size group; (2) in this study, the choice set in each period is binary, and the payoff structure is much simpler; (3) we have a third main treatment “Semi-gradualism,” which explores whether a sudden increase of stake harms coordination.

Romero (2011) studies the effect of path dependence on weakest-link coordination and finds that groups coordinate better with a certain cost when the cost is increasing than when the cost is decreasing to that level. This study differs from his in two ways: (1) we change the stake level, which indicates not only the cost but also the benefit; (2) we compare slow increasing of the stake with sudden increase and with starting at a high stake, while he compares an increasing path of the cost with a decreasing one.

The gradualism approach in this study is different from those in previous studies on dynamic real-time public goods provision by Dorsey (1992), Marx and Matthews (2000), Kurzban et al. (2001) and Duffy et al. (2007). Besides the binary weakest-link structure that differs from theirs, another feature of this study is that the “public projects” in each period are independent from each other (i.e., contributions cannot accumulate over periods) and each project has a target (stake) for itself; in their studies on dynamic voluntary contribution to a single public project, players are allowed to contribute whenever and as much as they wish and accumulate their contributions over the course of the project (there is no objective for each period before the end.) This study answers whether a group should work on smaller

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Kurzban et al. (2008) contradict this prediction by showing that subjects prefer starting with smaller levels of investment and increasing it, rather than the other way around.

tasks before accomplishing a large collective task, rather than whether a call for large collective contributions should be divided into multiple periods to allow accumulations over time. Although their studies fit many real world examples such as long-course fund drives, this study matches other important cases mentioned in the introduction: in these examples, the duration of the final high-stake project is relatively short and cannot divide into sub-periods to accumulate efforts, and there is no frequent feedback about others' contributions to the final project; instead, beside the final high objective, in each period there is an independent project with a clear smaller objective, and after each period players evaluate how they perform on these small tasks.

Our results show more clearly the efficiency gains of gradualism than the study by Andreoni and Samuelson (2006). They examine a twice-played prisoners' dilemma in which the total stakes in two periods are fixed, while the distribution of these stakes across periods can be varied. Both their theoretical and experimental results show that it is best to "start small," reserving most of the stakes for the second period. However, in their experiment cooperation is low for the period with a high stake,<sup>11</sup> which shows their gradualism setting actually does not largely help improve cooperation at a high stake and cannot serve as an effective tool to build cooperation.<sup>12</sup> Of course, the clearer effect of gradualism in our study may be due to the weakest-link structure which does not allow free riding.

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<sup>11</sup> When the relative stake of period 2 is high, there is more cooperation in period 1 but less cooperation in period 2; when the relative stake of period 2 is low, there is less cooperation in period 1 but more cooperation in period 2.

<sup>12</sup> One example (and motivation) in their study is as follows: "An employer forming a new team of workers may give them small initial tasks, to help build cooperation, followed by larger tasks that can take advantage of that cooperation." According to their results, actually the workers are very unlikely to reach successful cooperation for larger tasks.



Offerman and van der Veen (2010) study whether governmental subsidies to promote public good provision should be abruptly introduced or gradually increased, i.e., given the benefit of the public good, whether the individual cost of providing the public good should be decreased sharply or gradually. Their result favors an immediate increase of subsidy: when the final subsidy level is substantial, the effect of a quick increase is much stronger than that of a gradual increase. This study differs from theirs in the following important ways. First, their motivation is about how to use subsidies to stimulate cooperation after cooperation failures at the beginning. The mechanism in this chapter does not use governmental subsidies to promote cooperative behavior; instead it uses a low stake to facilitate earlier coordination and a gradual increasing stake path to try to sustain coordination at higher levels. Second, in this study, what may change is the stake level, which indicates both the cost and the benefit of the public good (in their study, only cost changes.) Third, stake paths in this study are non-decreasing, while their cost paths are non-increasing.

There are some studies on monotone games, multi-period games in which players are constrained to choose strategies that are non-decreasing over time, i.e., to increase contributions over time (e.g., Gale, 1995, 2001; Lockwood & Thomas, 2002; Choi et al., 2008). In contrast to these studies, this study employs another setting – forcing the stake instead of the contribution to be non-decreasing – to better match the aforementioned motivation examples.

In two theoretical papers, Watson (1999, 2002) shows “starting small and increasing interactions over time” is an equilibrium for dynamic cooperation. This study mainly adopts an empirical method and focuses on weakest-link coordination problems with no chance for

free riding. By determining the stake path exogenously, it answers whether gradualism works better, rather than whether players use a gradualism approach.

### **1.3. Experimental Design**

Our experiment is a minimum-effort coordination game with a much simpler payoff structure than the standard coordination games in the literature. Specifically, it is a multi-period stag hunt game due to the binary-choice feature in each period.

#### **1.3.1. Sample and Payoff Structure**

The laboratory experiment was conducted at Renmin University of China in Beijing, China in July 2010 with 256 subjects recruited via the Bulletin Board System (BBS) and posters at the university. Most subjects were students from this university and universities nearby, and people living nearby. Table 1.1 contains basic summary characteristics of the subject pool. The subjects are generally young with an average age around 22, since 91% of them are college or graduate students. 41% are male. 12% are (or were) majored in economics, 16% in other social sciences, 27% in business, and the remaining in other disciplines. The average individual annual income in the year of 2009 falls between 5,000 *yuan* and 10,000 *yuan*.

Table 1.1: Summary Statistics of Subjects' Survey Information

Variable	Mean and standard deviation	Observations
Age	22.05 (3.25)	255
Male	0.41 (0.49)	255
Income	1.32 (1.38)	255
Family Income	5.63 (2.69)	189
Family Economic Status	2.60 (0.74)	254
Risk Aversion Index	4.47 (1.80)	250
Han nationality	0.91 (0.29)	255
Student	0.91 (0.29)	255
<b>Concentration:</b>		
Economics	0.12 (0.33)	241
Other Social Sciences	0.16 (0.37)	241
Business	0.27 (0.45)	241
Humanity	0.12 (0.33)	241
Science	0.15 (0.35)	241
Engineering	0.17 (0.38)	241
Medical/Health	0.01 (0.09)	241

Note: Income is a scale variable from 0 to 13, with higher value indicating higher income (0: no income; 1: annual income<5000 *yuan*; 13: annual income>160,000 *yuan*). Family income is a scale variable from 1 to 12, with a higher value indicating a higher income (1: annual income<5000 *yuan*; 12: annual income>200,000 *yuan*). Family economic status is coded in the following way: 1 (lower), 2 (lower middle), 3 (middle), 4 (upper middle), 5 (upper). Risk aversion index is a scale from 0 to 10, with a higher value approximately indicating higher risk aversion, and is measured as the number of lottery A chosen by the subject in our questionnaire.

There were 18 sessions. All sessions were computerized using the z-Tree experiment software package (Fischbacher, 2007). Both the instructions and the information shown on the computer screen were in Chinese. In each session, we randomly assigned subjects to groups of four,<sup>13</sup> so our sample consisted of 64 groups in total. The experiment included two stages: the first stage comprised twelve periods, while the second one had eight periods. In each period, we endowed subjects with 20 points and asked them to give a certain number of points to their assigned groups' common pools. The required number could vary across

<sup>13</sup> For coordination games, four is considered as a small or moderate group size. For public goods games, Croson and Marks (2000) show in a meta-analysis study that the most frequently used group sizes are four, five, and seven.

periods, and each subject could only choose either “not to give” (we use the natural term “give” rather than “contribute” in the instruction) or “to give the *exact* points required,” which we refer to as *stake*. If all members in a group gave, then each member not only got the points she had given back, but also gained an extra return which equaled the required number of points (i.e., the stake). If not all group members contributed, then each member finished each period only with her points remaining, and the points she gave out were wasted.

In sum, the following formula describes individual earnings in each period:

$$Earning_{i,t} = \begin{cases} 20 + Th_t, & \text{if } A_{i,t} = C \text{ and } A_{j,t} = C, \forall j \neq i \\ 20, & \text{if } A_{i,t} = D \\ 20 - Th_t, & \text{if } A_{i,t} = C \text{ and } \exists j \neq i, \text{ s.t. } A_{j,t} = D \end{cases}$$

Where  $Earning_{i,t}$  is  $i$ 's earning in period  $t$ ,  $Th_t$  is stake at  $t$ .  $A_{i,t}$  and  $A_{j,t}$  are the actions of  $i$  and  $j$  at  $t$ , respectively ( $i$  and  $j$  are in the same group.)  $C$  represents “cooperate” (“give”), while  $D$  represents “deviate” (“not give”).

The final payment is the total earning accumulated over periods plus a show-up fee, and the exchange rate is 40 points per *yuan*.<sup>14</sup> An average subject earns 21~22 *yuan* (around three dollars) including the show-up fee from the whole experiment, which affords ordinary meals for one to two days on campus. Regarding the purchasing power, this payment is comparable to those experiments conducted in other countries.

### 1.3.2. Treatment Group Assignments

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<sup>14</sup> The *yuan/USD* exchange rate was about 6.7.

Our experiment consisted of three main treatments: “Big Bang,” “Semi-gradualism” and “Gradualism.” All groups in the three main treatments faced the same stake in the second half (periods 7-12) of the first stage, but the stake paths for them differed in the first half (periods 1-6), which may have imposed an income effect in the laboratory. To estimate how much of the difference in performance among the three main treatments in the second half of the first stage is driven by an income effect, we introduced a variant of the “Big Bang” treatment, namely the “High Show-up Fee” treatment, which we describe in more details below.<sup>15</sup> In eight of the 18 sessions, 12 subjects were randomly assigned into the three main treatments; in the remaining 10 sessions, 16 subjects were randomly assigned into the four treatments (three main treatments and one supplementary treatment). In total, we have 18, 18, 18 and 10 groups in “Big Bang,” “Semi-gradualism,” “Gradualism,” and “High Show-up Fee” treatments, respectively.

Table 1.2 checks how randomization worked in assigning subjects into different treatments. The default category is the “Gradualism” treatment and the regressions do not have other control variables, so the constant term indicates the mean values of dependent variables for the “Gradualism” treatment. There are no significant differences in subjects’ characteristics across treatments, except that subjects in the “Semi-gradualism” treatment have higher self-reported family economic status, that those in the “Big Bang” and “High Show-up Fee” treatments have higher risk aversion indexes, and that those in the “Big Bang” treatment are more likely to be students. This shows that randomization did very well.

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<sup>15</sup> The experimental literature often adopts a random period for payment to address the concern of income effect. We did not follow this design due to the concern that it may make some subjects less serious at playing in each period, and we also want to capture how big the income effect is.

**Table 1.2: Comparison of Subjects' Characteristics by Treatment**

	Dependent Variable								
	Age	Male	Income	Family Economic Status	Risk Aversion Index	Han Nationality	Student	Economics Major	Business Major
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
BIG BANG	-0.306 (0.612)	-0.042 (0.081)	-0.125 (0.241)	-0.083 (0.123)	0.474* (0.279)	0.028 (0.046)	0.083* (0.046)	0.026 (0.056)	0.006 (0.078)
SEMI-GRADUALISM	-0.278 (0.627)	0.076 (0.083)	-0.360 (0.236)	0.217* (0.128)	0.424 (0.295)	0.013 (0.048)	0.055 (0.050)	-0.000 (0.053)	0.030 (0.080)
HIGH SHOWUP FEE	-0.111 (0.695)	0.086 (0.098)	0.056 (0.336)	-0.197 (0.146)	0.795** (0.399)	-0.028 (0.063)	-0.050 (0.072)	0.053 (0.071)	-0.126 (0.081)
Constant	22.236*** (0.547)	0.389*** (0.058)	1.444*** (0.204)	2.597*** (0.094)	4.097*** (0.197)	0.903*** (0.035)	0.875*** (0.039)	0.104*** (0.038)	0.284*** (0.056)
Observations	255	255	255	254	250	255	255	241	241
R-squared	0.00	0.01	0.01	0.04	0.02	0.00	0.03	0.00	0.01

Note: The default treatment is "Gradualism." Robust standard errors in parentheses. \* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%. For measures of variables, see note in Table 1.1.

In the first stage, the stakes over 12 periods are shown in Figure 1.1: for the “Big Bang” treatment, the stakes are always at the highest level, which is 14 for 16 sessions and 12 for two sessions;<sup>16</sup> for the “Semi-gradualism” treatment, they are two for the first six periods and set at the highest stake for the next six periods; for the “Gradualism” treatment, they increase from two to 12 with a step of two for the first six periods and fix at the highest stake for the next six periods. (Revisit Figure 1.1 for a graphical illustration.) The show-up fees for these three treatments are 400 points for each individual. The “High Show-up Fee” treatment is the same with the “Big Bang” treatment, except that the show-up fee is 480 instead of 400 points. The extra 80 points are sufficient to capture potential earning differences accumulated from periods 1-6 and thus to isolate the effect generated by an income effect by comparing the “High Show-up Fee” to the “Big Bang” treatment (we discuss this in detail in Section 1.4.)

When subjects enter the second stage of the game, they are randomly reshuffled into groups of four. New group members may not necessarily come from the same treatment in the first stage; this rule is made common knowledge. Within the second stage, group compositions are fixed, and stakes are all set at the highest stake for all periods and all groups, i.e., those treated in different treatments in the first stage face the same stake in each period of the second stage.

The information structure is as follows.

Subjects know that there are two stages, but not the exact number of periods in each stage. Instead, they are told that the experiment will last between 30 minutes and one hour, including the time for signing up, reading of instructions, a quiz designed to make sure that

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<sup>16</sup> We calibrated the highest stake level using 12 and 14, and finally decided to choose 14 in most sessions. To make full use of the samples, in the following analysis we pool all 18 sessions together.

they understand the experimental rule, and final payment. We chose this design for two reasons. First, it is to reduce the possibility of backward induction in theory<sup>17</sup> and a potential end-of-game effect.<sup>18</sup> Second, in many cases of the real world, people do not know the exact number of coordination opportunities (e.g., how many times they will meet each other, how many projects they will have).

At the beginning of each period they know the stake of the current period but not those of future periods. In many cases of the real world, the levels of future interactions are unknown *ex ante*.

At the end of each period they know whether all four group members (including herself) gave the required points of that period, but not the total number of group members who gave (if fewer than four members gave). This is consistent with the literature of minimum-effort coordination games (e.g., Van Huyck et al., 1990), in which the only common historical data available to players is the minimum.<sup>19</sup> This setting is also popular in the literature of contract theory, in which imperfect observation of efforts is common. By adopting this design we can also increase the difficulty of coordination given other aspects of the experiment.

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<sup>17</sup> Although the number of periods is unknown in our experiment, it is finite. So theoretically players can still conduct backward induction to some extent. However, backward induction is not well supported empirically in the literature.

<sup>18</sup> In minimum-effort coordination games the end-of-game effect should be absent or minor since there is no incentive to free ride given that others cooperate.

<sup>19</sup> In our setting, if all members cooperate, then the minimum is the stake (or coded in a binary way, “1”); otherwise the minimum is zero.



Communication across players is not allowed. We chose this design for two reasons. First, communication is often impossible or ineffective in the real world. Second, this design makes coordination more difficult.<sup>20</sup>

At the start of the second stage, each player is notified that they enter a new random group. At the end of each stage, each player is told how many points she has accumulated to date.

At the end of the experiment, we asked subjects to complete a brief survey. The survey collected information on age, gender, nationality, education level, concentration at school, working status and income, in addition to eliciting risk preferences over lotteries (see Appendix A.2).

## 1.4. Results

In this section we present our findings in summary tables, figures and regressions. We analyze the effect of treatments on the following three outcome variables per period: whether a group coordinates successfully, whether an individual contributes, and the individual's earning.

Table 1.3 contains the summary of designs and performances of all four treatments in the first stage. Clearly, the “Gradualism” treatment has better performances of coordination for periods 7-12 in the first stage, which shows that gradualism does promote coordination at a high stake level. For example, in period 7, 66.7% of “Gradualism” groups have coordinated successfully, while the success rates of “Big Bang,” “Semi-gradualism,” and

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<sup>20</sup> Ostrom (2010) summarizes that communication improves cooperation. Charness (2000) shows that communication helps coordination in small groups, while Weber et al. (2001) and Chaudhuri et al. (2009) find that large group coordination is still difficult even with communication.

“High Show-up Fee” groups are only 16.7%, 33.3% and 30%, respectively. Similar results can be found in period 12.

**Table 1.3: Summary of Treatments in the First Stage**

Treatment	Big Bang	Semi-gradualism	Gradualism	High Show-up Fee
Endowment in each period	20	20	20	20
Show up Fee (points)	400	400	400	480
Exchange Rate (points/yuan)	40	40	40	40
Stake in period 1	14*	2	2	14*
Stake in period 6	14*	2	12	14*
Stake in period 7-12	14*	14*	14*	14*
Number of groups	18	18	18	10
Number of subjects	72	72	72	40
Number of groups successful in period 1	3	13	13	3
Number of groups successful in period 7	3	6	12	3
Number of groups successful in period 12	3	6	11	3
Percent of groups successful in period 1	16.7%	72.2%	72.2%	30%
Percent of groups successful in period 7	16.7%	33.3%	66.7%	30%
Percent of groups successful in period 12	16.7%	33.3%	61.1%	30%
Average earning up to Period 6 (show-up fee not included)	112.42	126.31	143.94	127.35
Median earning up to Period 6 (show-up fee not included)	106	130	162	106

\*: In two sessions, it is 12 rather than 14.

To check the concern that the different performances for periods 7-12 may be due to an income effect (i.e., those treated in the “Gradualism” treatment may earn more in periods 1-6, so they are more likely to contribute in periods 7-12), Table 1.3 also provides summaries of individual earning up to period 6 (i.e., earning accumulated from period 1 to 6, not including the show-up fee) for each treatment. On average, subjects in the “Gradualism” treatment do have the highest earning from the first six periods: the average (median) earning by period 6 is 112.42 (106) for the “Big Bang” treatment, 126.31 (130) for the

“Semi-gradualism” treatment, and 143.94 (162) for the “Gradualism” treatment. But the differences in means (and medians) are much smaller than 80 points, the difference of show-up fee between the “High Show-up Fee” treatment and the other three treatments. This shows that a show-up fee difference of 80 points between the “Big Bang” and “High Show-up Fee” treatments is large enough to capture the potential income differences at the start of period 7 between “Big Bang,” “Semi-gradualism,” and “Gradualism” treatments. Actually, if we add up the show-up fee, subjects in the “Gradualism” treatment earn less than those in the “High Show-up Fee” treatment on average by period 6. So since we still find the “Gradualism” treatment has better performance than the “High Show-up Fee” treatment in periods 7-12 in stage 1, the difference should not be driven by a potential income effect from the first six periods.<sup>21</sup>

**Result 1: The success rate in period 1 is higher for groups with a lower stake.**

In period 1, over two third of “Semi-gradualism” and “Gradualism” groups coordinate successfully at the low stake, while only three out of 18 (or 10) of the “Big Bang” (or “High Show-up Fee”) groups are successful at the high stake.

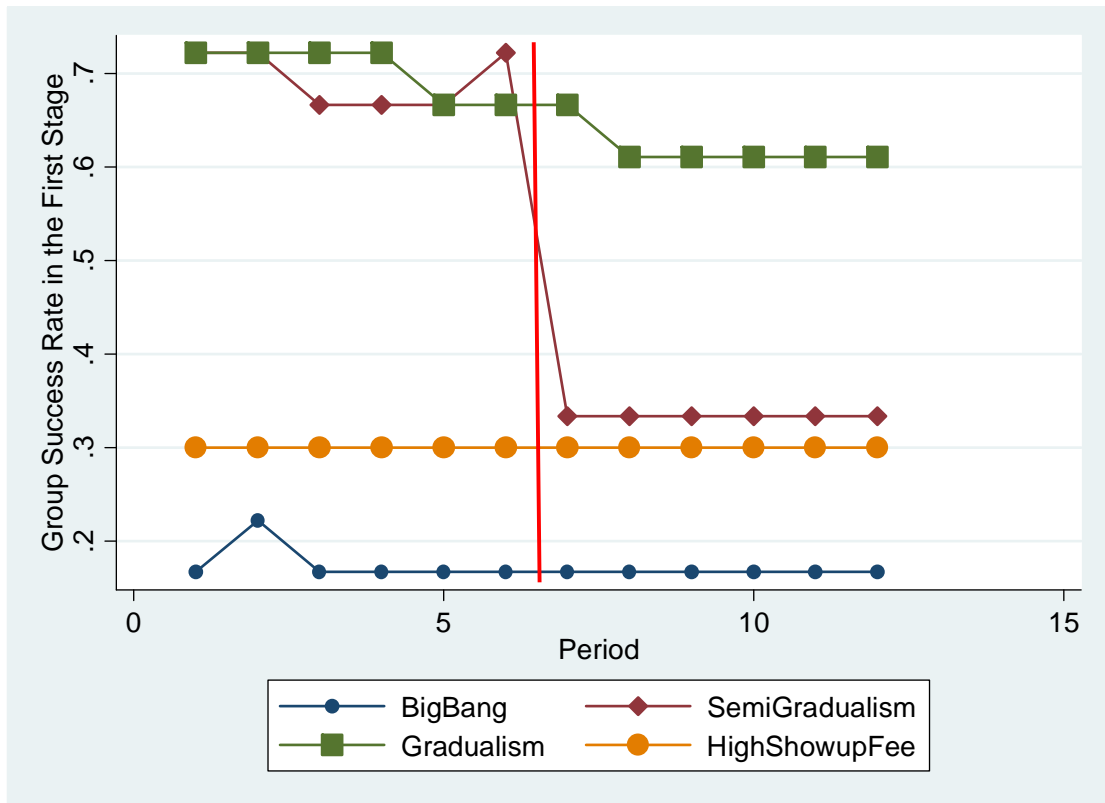
**Result 2: For groups that have attained successful coordination at  $t$  with the same stake, the success rate at  $t+1$  is higher for those with a lower increase of stake at  $t+1$ .**

This pattern can be seen in Table 1.3 and Figure 1.2. A large gap between “Gradualism” and “Semi-gradualism” treatments emerges in period 7 when the stake jumps from two to 14 for the “Semi-gradualism” treatment. Both treatments have high success

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<sup>21</sup> Real world income may also affect individuals’ decisions. By randomly assigning subjects into various treatments, we rule out the possibility that the differences of performance are due to real world income.

rates of approximately 70% for the first six periods. But the success rate of the “Semi-gradualism” treatment decreases sharply to only 33.3% from period 7, while that of the “Gradualism” treatment remains at a high level of above 60%. The “High Show-up Fee” treatment has a success rate of 30% for all 12 periods, and about 16.7% of “Big Bang” groups succeed for almost all periods.



A group is successful if all four members give the required amount (stake) in that period.

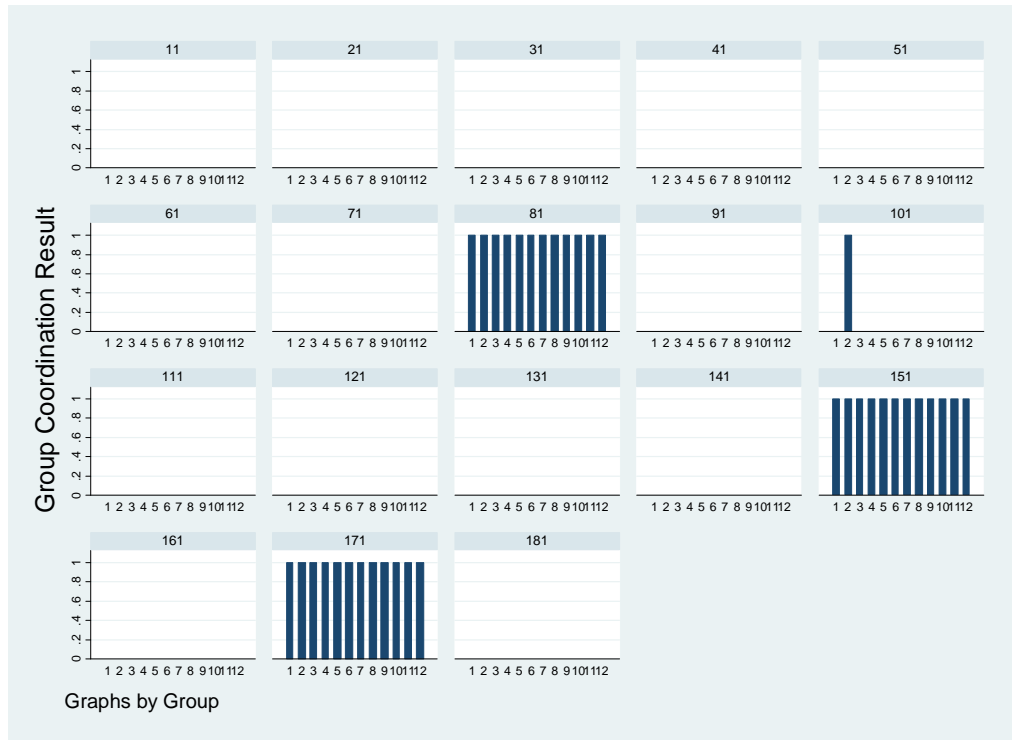
Figure 1.2: Success Rates of Groups by Treatment and Period in the First Stage

**Result 3: Conditional on failed coordination at  $t$ , most groups fail with the same or a higher stake at  $t+1$ .**

In Figure 3.1 we examine how coordination results vary across periods for each group by each treatment. For each group, the horizontal axis indicates the period, and the vertical axis indicates the coordination result (success or failure): a bar with value one indicates

successful coordination, while that with value zero indicates failed coordination. Each group is identified by a code in the following way: the lowest digit indicates the treatment type (1=“Big Bang,” 2=“Semi-gradualism,” 3=“Gradualism,” 4=“High Show-up Fee”); the highest one or two digits indicate the session number (1-18).

Figure 1.3 clearly shows that once a group has failed in coordination, it almost never becomes successful subsequently. There are only five exceptions (one “Big Bang” group and four “Semi-gradualism” groups) out of 64 groups. This finding is consistent with Weber et al. (2001) and Weber (2006), in which they find once groups have reached an inefficient outcome, they are unable subsequently to reach a more efficient outcome.<sup>22</sup>



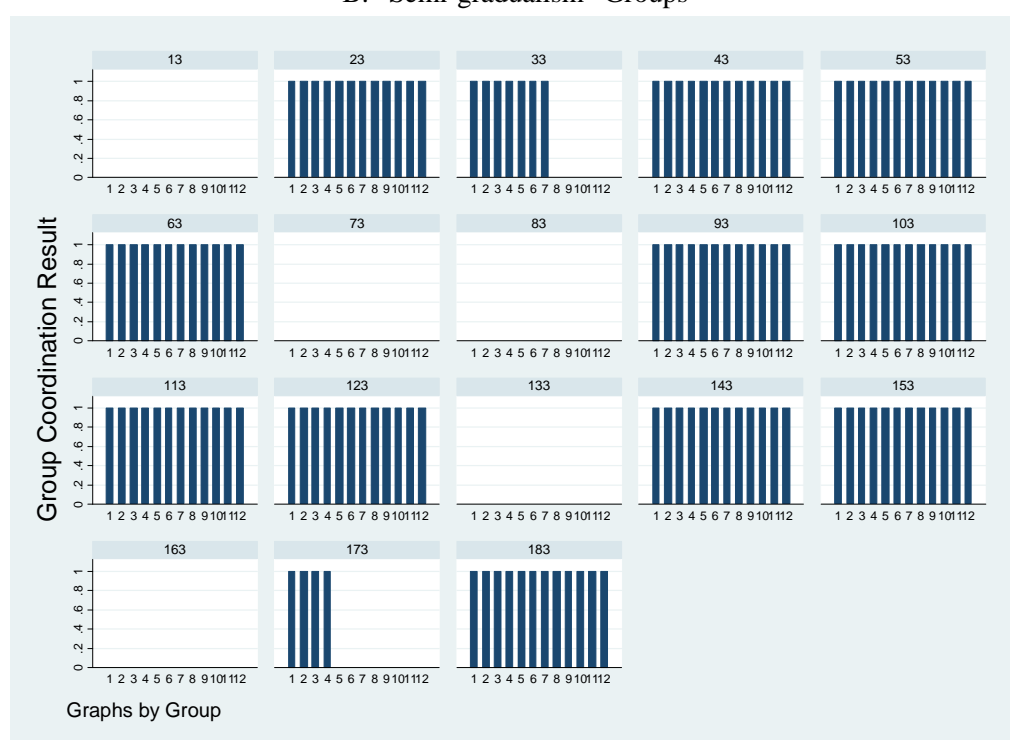
A: “Big Bang” Groups

Figure 1.3: All Group Coordination Results for Each Treatment

<sup>22</sup> However, changing incentives can improve coordination (Berninghaus & Ehrhart, 1998; Bornstein, Gneezy & Nagel, 2002; Brandts & Cooper, 2006b).

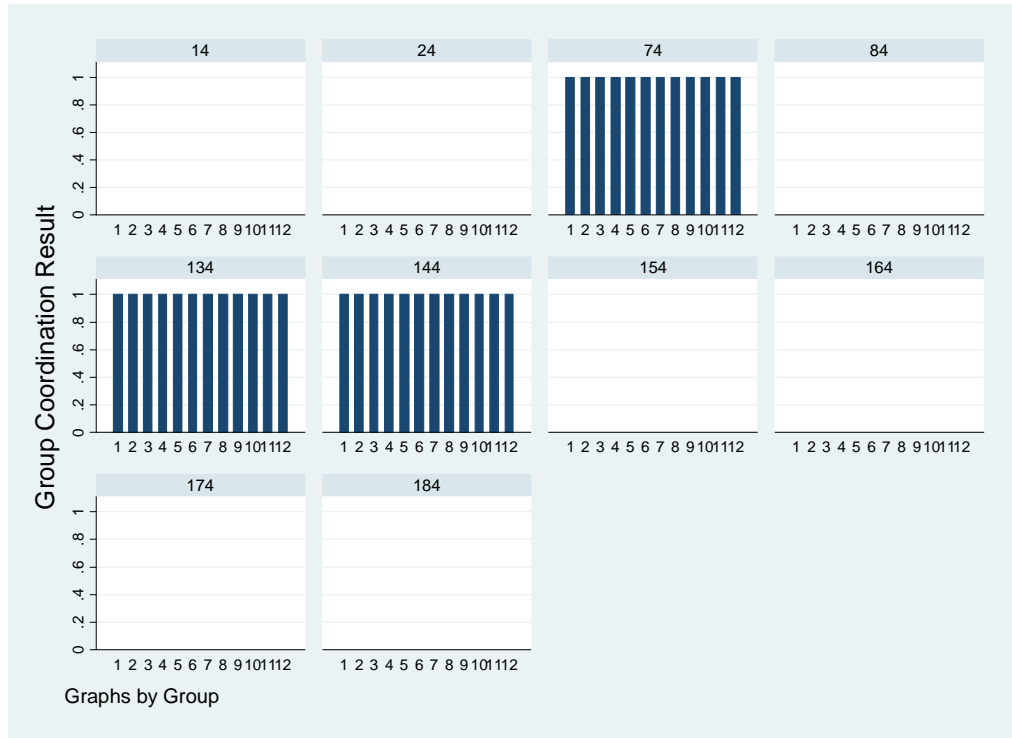


### B: “Semi-gradualism” Groups



### C: “Gradualism” Groups

Figure 1.3 (Continued)



D: “High Show-up Fee” Groups

Figure 1.3 (Continued)

**Result 4: Conditional on successful coordination at  $t$ , most groups succeed with the same stake at  $t+1$ .**

Similarly, once a group has succeeded, it almost always remains successful with the same stake. Figure 1.3 shows that there are only seven exceptions (one “Big Bang” group, four “Semi-gradualism” groups, and two “Gradualism” groups). Conditional on successful coordination in period 1, “Big Bang,” “Gradualism” and “High Show-up Fee” groups are almost all successful in following periods; but since the “Big Bang” and “High Show-up Fee” treatments have a much lower success rate in period 1 than the “Gradualism” treatment, on average they perform much worse than the “Gradualism” treatment at a high stake.

Figure 1.4 shows the fractions of individuals contributing over periods in the first stage for the four treatments. “Big Bang” and “High Show-up Fee” treatments start with average contribution rates above 60%, but decrease quickly over the initial periods and end at about 20% and 40%, respectively. “Semi-gradualism” and “Gradualism” treatments start with a high average contribution rate above 90%, decrease slightly over the first 6 periods to a rate of about 80%. However, the contribution rate decreases sharply from period 6 to 7 for the “Semi-gradualism” treatment (when stake is sharply raised from two to 14), while that for the “Gradualism” treatment remains high. Although the contribution rates of all treatments generally decrease over time (even for the “Gradualism” treatment), the results are consistent with Figures 1.2 and 1.3: except for several exceptions and for “Semi-gradualism” groups (from period 6 to 7), the decrease of contribution rates is almost all caused by those who give up cooperating when their cooperative actions at previous periods have not been rewarded because of coordination failure, while those who have succeeded keep cooperating. The differences among the four treatments in Figure 1.2 (regarding group success rate) is much more stark than those in Figure 1.4 (regarding fractions of individuals contributing), since you need all four members to give at the same time to make the group coordination successful, which is much more difficult than asking only one person to give. This is why coordinating at the same pace is so important, and gradualism helps address this challenge significantly.



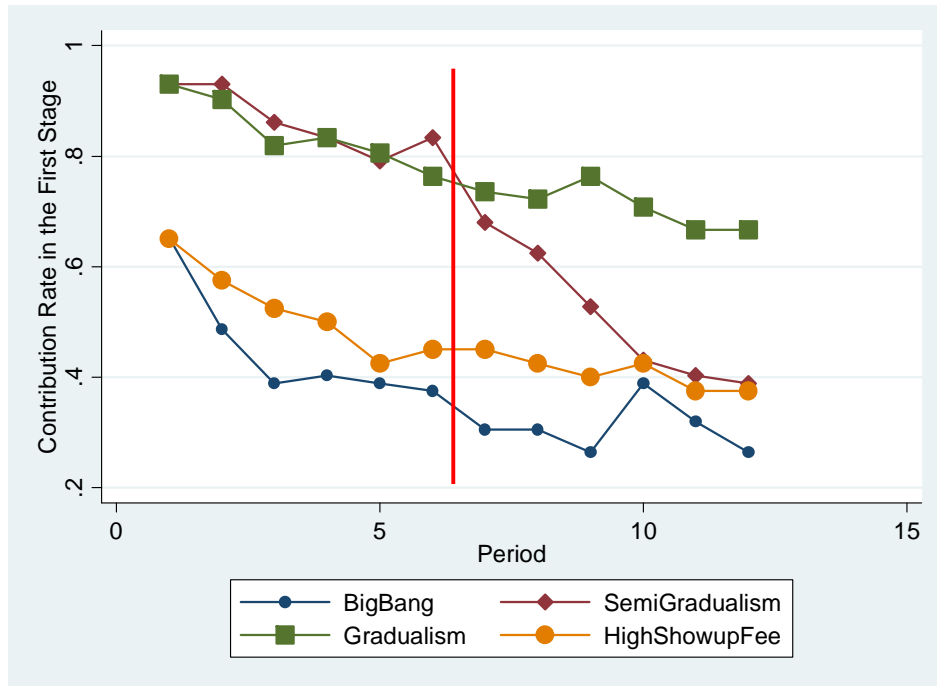


Figure 1.4: Contribution Rate by Treatment and Period in the First Stage

Figure 1.5 shows the average individual earning in each period. The results also show that the “Gradualism” treatment works best through the perspective of social welfare.

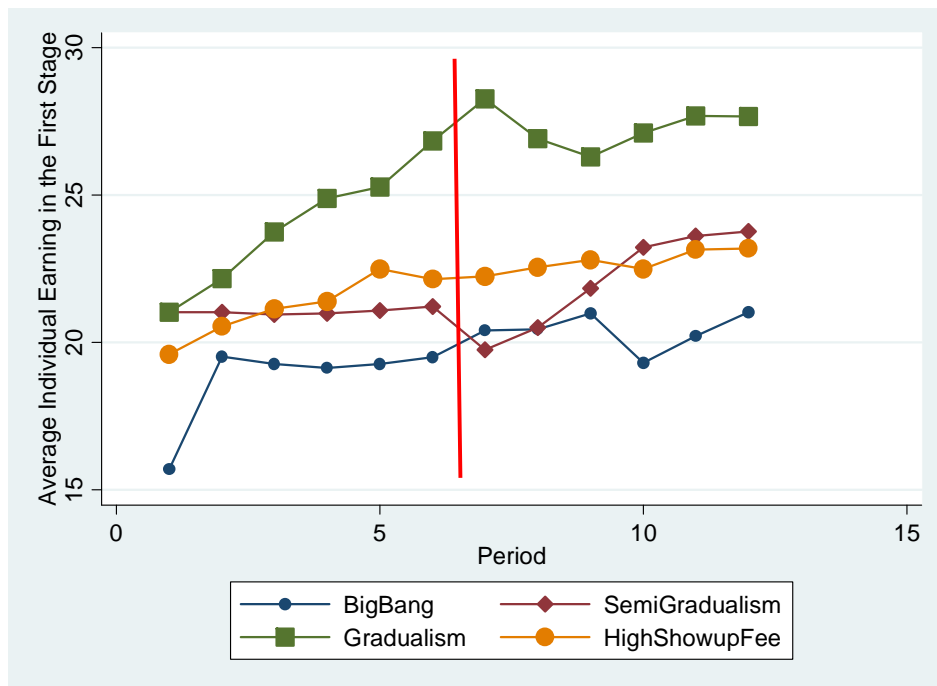


Figure 1.5: Average Individual Earning by Treatment and Period in the First Stage

Table 1.4 shows the formal regression results for periods 7-12 in the first stage, when all treatment groups face the same high stake. The default category is the “Gradualism” treatment and the regressions do not have other control variables, so the constant terms indicate the mean values of dependent variables for the “Gradualism” treatment. Dependent variables are a dummy indicating whether an individual gives or not in periods 7 and 12 (in Column 1 and 2, respectively), an individual’s earning in periods 7 and 12 (in Column 3 and 4), and a dummy indicating whether the group has successful coordination: we consider period 7 in Column 5, period 12 in Column 6, and since this dummy is our main variable of interest, we further examine all six periods from period 7 to 12 in Column 7. All standard errors are clustered at the appropriate level. It shows that the differences between the “Gradualism” treatment and other treatments are mostly large and significant. F-test (unreported) shows that the differences between “Big Bang” and “High Show-up Fee” treatments are statistically insignificant, although it might be due to a relatively small sample. Moreover, the large and significant differences between “Gradualism” and “High Show-up Fee” treatments suggest that the advantage of gradualism is not driven by the income effect in the laboratory. We also employ probit and logit specifications (unreported) when the dependent variable is whether an individual contributes or whether a group reaches successful coordination, and the results are very similar with those OLS results in Table 1.4 (Columns (1), (2), and (5)-(7)). When the dependent variables are contribution and earning, which are at the individual level, additional regressions (unreported) with survey controls show very similar results with those in Columns (1)-(4).<sup>23</sup>

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<sup>23</sup> There may be a concern that economics and business students play differently with other students. We address this concern by examining actions in the first period, as well as actions in the second period conditional on the coordination outcome of the first period, and find no differences between economics/business students and other students.

**Table 1.4: Contribution, Earning and Success in Periods 7-12 of the First Stage by Treatments**

	Individual Contribution			Individual Earning			Success (Group-level)		
	Period 7	Period 12		Period 7	Period 12		Period 7	Period 12	Period 7-12
	(1)	(2)		(3)	(4)		(5)	(6)	(7)
BIG BANG	-0.431*** (0.126)	-0.403*** (0.136)		-7.861*** (2.472)	-6.639*** (2.401)		-0.500*** (0.171)	-0.444** (0.170)	-0.454*** (0.144)
SEMI-GRADUALISM	-0.056 (0.113)	-0.278* (0.152)		-8.528*** (3.076)	-3.889 (2.587)		-0.333* (0.185)	-0.278 (0.182)	-0.287* (0.157)
HIGH SHOWUP FEE	-0.286* (0.158)	-0.292 (0.176)		-6.028* (3.203)	-4.467 (3.049)		-0.367* (0.199)	-0.311 (0.201)	-0.320* (0.186)
Constant	0.736*** (0.092)	0.667*** (0.107)		28.278*** (1.909)	27.667*** (1.925)		0.667*** (0.117)	0.611*** (0.121)	0.620*** (0.114)
Observations	256	256		256	256		64	64	384
R-squared	0.13	0.10		0.13	0.10		0.16	0.13	0.13

Note: The default treatment is "Gradualism." Robust standard errors in parentheses. \* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%. Individual contribution and group-level success are binary variables. Standard errors are clustered at session level for (5) and (6), and clustered at group level for all other regressions. The observations are at individual level for regression (1)-(4), and at group level for regression (5)-(7). There are 256 subjects in 64 groups of 18 sessions. When we add survey controls for regression (1)-(4) where the observations are at individual level, the results (unreported) remain similar.

**Result 5: Those treated in the “Gradualism” treatment in stage 1 are more likely to contribute upon entering a new group in stage 2. But this difference quickly disappears.**

In Table 1.5 we examine whether the treatments in the first stage have effects on behaviors and outcomes in the second stage when subjects enter a new group. Note that everyone knows that the new group members may have been exposed to other stake paths in the first stage. Since the group compositions are different from those in the first stage, we only look at individuals’ contribution and earning in each period of the second stage, rather than the group-level coordination results. Interestingly, those in the “Gradualism” treatment in the first stage are more likely to contribute in the first period of the second stage. It is important to keep in mind that group assignment in stage 2 is a new randomization, and subjects know that they are playing in a new group. Thus, this finding is indicative of a new effect, namely an inter-stage effect of having been treated in a gradualism environment, which is similar with the inter-stage effect in trust games found by Bohnet and Huck (2004).<sup>24</sup> However, this effect disappears over the course of the second stage, suggesting a learning process where the behaviors of the different treatment groups converge as they observe the play of their new group members. For example, those in the “Gradualism” treatment find their new group members are not as cooperative as those in the first stage, thus becoming less willing to contribute in following periods.

These findings suggest that the gradualism setting can induce a long-run cooperative behavior, most likely through a history of successful coordination and an increased level of

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<sup>24</sup> They study a trust game and find that trustees are more trustworthy in the stranger environment in the second stage after having been exposed to a partner in the first stage.

trust.<sup>25</sup> However, when they find their trust does not get rewards in a new environment, they tend to become less cooperative. This shows an externality of coordination building (or collapse) across different social groups.

Table 1.5: Contribution and Earning in Each Period of the second Stage by Treatment

	Period 1		Period 2		Whole Stage	
	Contribution	Earning	Contribution	Earning	Contribution	Earning
	(1)	(2)	(3)	(4)	(5)	(6)
BIG BANG	-0.125* (0.069)	1.639 (1.764)	0.000 (0.073)	0.389 (1.524)	0.002 (0.052)	0.514 (1.143)
SEMI-GRADUALISM	-0.097 (0.072)	3.111 (1.890)	0.000 (0.091)	1.833 (1.500)	0.066 (0.066)	1.240 (1.297)
HIGH SHOWUP FEE	-0.161* (0.089)	-1.467 (2.438)	0.053 (0.091)	-5.100** (2.230)	-0.096 (0.071)	-2.447 (1.541)
Constant	0.861*** (0.047)	18.17*** (1.895)	0.597*** (0.064)	21.75*** (1.493)	0.505*** (0.058)	22.47*** (1.195)
Observations	256	256	256	256	2,048	2,048
R-squared	0.020	0.018	0.002	0.044	0.011	0.016

Note: The default treatment is “Gradualism.” Robust standard errors in parentheses. Standard errors are all clustered at group level. \* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%. Individual contribution is a binary variable.

## 1.5. Conclusion and Discussion

The findings in this chapter suggest that gradualism -- defined as increasing step-by-step the stake level required for coordination -- can serve as a powerful mechanism for achieving socially optimal outcomes in coordination. In a laboratory setting, gradualism significantly outperforms alternative paths to coordinated behavior, which shows that starting at a low level and growing slowly are both important for later coordination at a substantial level. We also find an externality of coordination building (or collapse) across different social groups. Those treated in the gradualism setting are more likely to cooperate

<sup>25</sup> It is possible that those in the “Gradualism” treatment contribute significantly more in first period of the second stage just because of a wealth effect coming from their higher average earning from the preceding stage. Under the assumption of decreasing absolute risk aversion, this greater wealth could produce a greater willingness to contribute. While possible, we do believe that any income effect would be small, certainly not enough to generate the observed discrepancy.

upon entering a new environment than those treated differently. However, when they find their cooperation does not get rewards in a new environment, they tend to become less cooperative as well.

In this experiment we focus on certain settings of gradualism. There are many ways to extend this study in the future. Allowing communication, adopting a non-weakest-link payoff structure (e.g., allowing free riding), changing the group size and the highest stake level, adopting a different information structure and a more complex dynamic path of stakes, are all important dimensions for future study. It will be also interesting to examine whether gradualism helps rebuild coordination after it collapses, and whether an end-of-game effect exists and affects the role of gradualism in coordination.

Compared to the existing literature on coordination games, this chapter also suggests a case where limiting choices can improve social welfare. The literature on coordination games generally uses a complex payoff structure in which each individual has as many as seven choices for actions in each period (e.g., Van Huyck et al., 1990; Knez & Camerer, 1994, 2000; Cachon & Camerer 1996; Weber, 2006; Chaudhuri et al., 2009). For those with higher willingness to give in the first period, once they observe their high-cooperative actions are harmed by low-cooperative actions of their partners, they reduce the level of cooperation. After just several periods, an inefficient outcome is attained rather than a high efficient outcome, and the groups are then trapped in this low equilibrium. In contrast, in our experiment the subjects are restricted to two choices in each period: giving a specified amount or not giving. A gradualism institution, which increases the specified amount gradually, maintains subjects' high willingness to give even when the specified amount becomes substantial. As suggested in this study, limiting choices in each period (but without

mandatory or semi-mandatory institutions, e.g., sanction and social pressure), plus a well-designed institutional path, may help subjects reach a social optimal outcome.<sup>26</sup> Questions about the optimal path to attain a long-run objective of collective optimal deserve future studies.

To the best of our knowledge, our study is the first one that clearly tests the role of exogenous gradualism in coordination within a given group. It adopts an exogenous setting of stake path from a normative perspective. The results have important implications for future research on real world policies to promote coordination among individuals, organizations, regions and countries.

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<sup>26</sup> In the gradualism treatment of our experiment, we limit the number of choices in each period to two (i.e., each one can choose not to give or to give exactly the stake points in each period). Since our experiment involves collective interactions and thus a more obvious externality of own action on others, it is less surprising that limiting individual choices may be good for social welfare. What is more interesting in our findings is that individuals still have the freedom to choose between two options in each period (i.e., they are not forced to cooperate), and there is no sanction, punishment, and social pressure.

## **2. GRADUALISM, WEAKEST LINK AND INFORMATION: THEORY AND COORDINATION EXPERIMENTS**

### **2.1 Introduction**

This chapter extends Chapter 1 (Ye et al., 2012) and tests the gradualism hypothesis under various information and payoff structures. I first provide theoretical predictions, and then compare three computer-based laboratory experiments with repeated interactions which stylize typical coordination settings in the real world. The detail of the first experiment is reported in Chapter 1, while this Chapter focuses on comparisons of the three experiments.

As introduced in Chapter 1, in each period of the experiments, subjects are endowed with some points (monetary units in the laboratory) and are asked to participate in a group project with a *stake*. They then choose whether to participate or not. If they participate, they contribute an amount of points equaling the stake; if not, they contribute zero. The decision in each period is binary: to contribute exactly the stake or nothing to the group project, but not a third amount. The path of the stake, which may or may not change over periods, is determined by the assigned treatment.

In the first two experiments, each member realizes an extra return only when all group members contribute to the project; otherwise each ends up with only the points that she does not give. These first two experiments differ from each other in their information structures. The information feedback in the first experiment is limited: after each period subjects are only informed whether all group members contribute. On the contrary, that in the second one is richer: they are told exactly how many group members contribute.



Coordination and cooperation projects in the real world not always have a weakest-link payoff structure: in many cases a project only requires part of the parties to contribute, so free riding is possible. To find how gradualism works with a non-weakest-link payoff structure, I also conduct a third experiment in which the project in each period can be successful as long as at least three (out of four) group members contribute. Other features of this experiment are identical to the second weakest-link coordination experiment described above.

Following the first experiment (introduced in Chapter 1), each of the other two experiments has three main treatments (“Big Bang,” “Semi-gradualism” and “Gradualism”) of stake patterns. (See Figure 2.1 for a graphical illustration.)

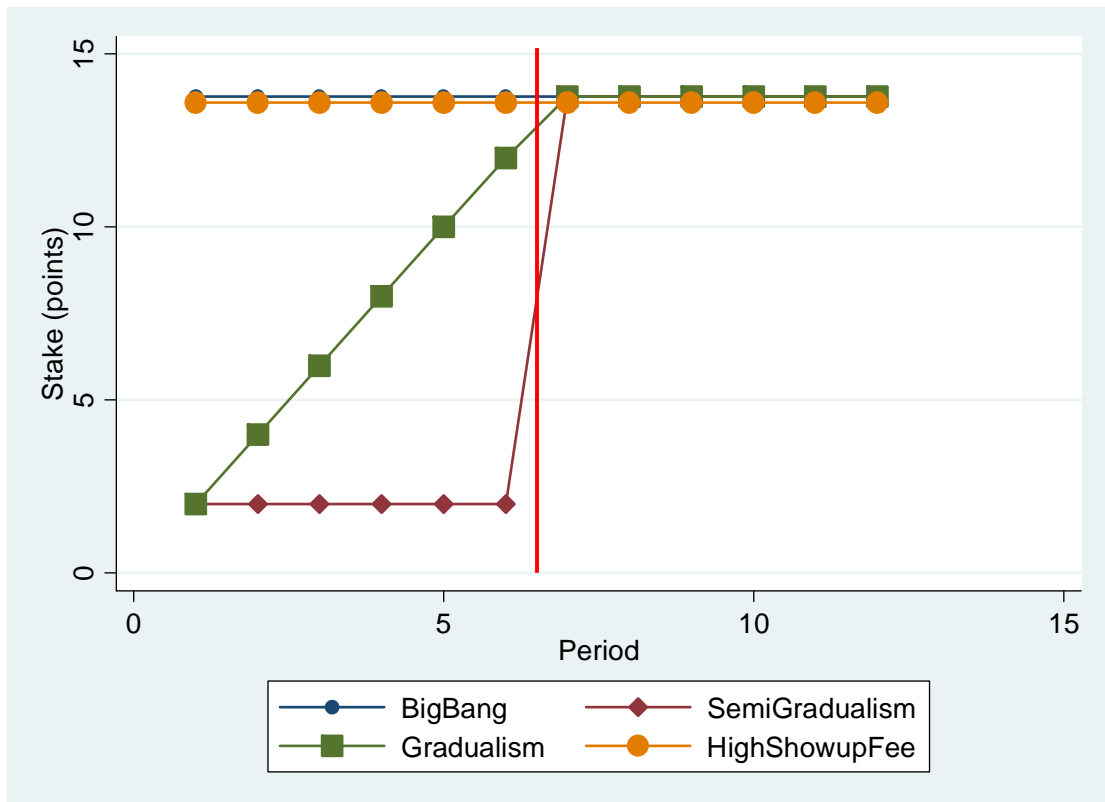


Figure 2.1: Stake Patterns of the Treatments

Comparing the two weakest-link experiments, I find that with limited information, individuals coordinate most successfully at the high stake level in the gradualism treatment relative to the *big-bang* approach; with a richer information structure, the advantage of gradualism shrinks because a richer information structure facilitates later coordination for the *big-bang* approach when a group is close to success. Finally, I find that when free riding is possible, all treatments perform worse and gradualism alone (without perfect monitoring and other institutions) does not help.

This chapter builds on Chapter 1 and explores the heterogeneous effect of gradualism under different information and payoff structures. It also adds to the literature on the impact of information structure in coordination, on discrete public good games,<sup>27</sup> and on the relationship between cooperation and coordination.<sup>28</sup> Detailed comparison to the literature on dynamic games can be found in Chapter 1.

The rest of the paper is organized as follows. In Section 2.2, I present theoretical predictions. In Section 2.3, I detail the experimental designs. In Section 2.4, I present the major results of the two weakest-link experiments. Section 2.5 further discusses the theoretical explanations for weakest-link games. Section 2.6 presents the third experiment with the possibility for free riding. Section 2.7 concludes.

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<sup>27</sup> For example, Palfrey and Rosenthal (1984), Isaac et al. (1989), Bagnoli and Lipman (1989), Bagnoli and McKee (1991), and Marks and Croson (1998, 1999). See Ledyard (1995) for an early survey.

<sup>28</sup> All three experiments in this chapter can be considered as either discrete public good games or coordination games, although the first two are generally referred as “coordination games” and the third a “discrete public goods game.” The only difference is about the required number of contributors (i.e., whether free riding is possible). This study suggests a clear link and the performance difference between these two types of games. Knez and Camerer (1994, 2000) discuss the relationship between prisoners’ dilemma and coordination games, and notice that organizational growth may switch a (weakest-link) coordination problem into a cooperation problem (with a chance for free riding).

## **2.2 Theoretical Predictions**

### **2.2.1 A Model of Belief Updating in Weakest-link Coordination**

Coordination problems involve multiple equilibria, thus belief about others' actions is a most important factor deciding which equilibrium to play in coordination. The starting stake level and the stake path can affect belief formation and updating; in addition, information and payoff structures shape beliefs directly, thus may affect coordination and the role of gradualism. Although the main contribution of this study is on the empirical side, I will first present a simple belief-based learning model for weakest-link coordination games to theoretically explore the potential role of gradualism in these games; in Section 2.2.2 I discuss the case when free riding is possible.

The intuition of the model is as follows. In the belief-based learning framework, players have prior beliefs about others' actions before the game starts, and update their beliefs according to the outcome in each period. A low stake at the beginning gives them stronger beliefs that others will try to cooperate, and also makes it cheap to attempt coordination in the face of uncertainty, so they will cooperate as well given the weakest-link payoff structure. Similarly, at each level where they manage to coordinate, they reinforce their beliefs about the likelihood that others will cooperate at the same or a slightly higher stake. On the contrary, if the stake starts at a high level, they believe that others are less likely to cooperate, which leads to initial failure and reduces their beliefs that others will cooperate later at the same or a higher stake level, thus undermining future coordination. Finally, in the presence of a sudden increase in the stake in two consecutive periods, previous coordination at low stakes may not largely affect players' posteriors on actions at

substantially higher stake levels; thus, successful coordination at a low level may not guarantee immediate successful coordination at a high level. In addition, information feedback is important for belief updating: a richer information structure makes belief updating easier, thus may facilitate coordination when (they know) most group members cooperate.

The main aspects of the model are belief-based learning, level-k thinking, myopia, and standard self-interested preferences with risk aversion. These assumptions allow me to focus on the belief updating process, a most important feature in coordination. A more general learning model of dynamic games is beyond the scope of this chapter.

I adopt the myopic assumption for two reasons. First, it is often used in learning models (e.g., reinforcement learning, belief-based learning, experience-weighted attention learning, adaptive dynamics; see Section 2.5.3 for details). Second, it does not affect the explanatory power of this simple model, and allows me to focus on the key process of belief updating. I will further discuss this in Section 2.5.1.

The setup of the model is as follows. There are  $N$  periods. In each period a group of  $I$  players conduct a binary coordination task: each player can choose to participate (cooperate, “C”) or not participate (deviate, “D”). The endowment per person in each period is  $E$ . The level of the coordination task,  $Th_t$  ( $Th_t > 0$ ), may vary across periods. So in each period, each player can choose to contribute either zero or exactly  $Th_t$ , but not other amounts of efforts, to the group project. I adopt a minimum-effort (weakest-link) payoff structure: the value of the project output for everyone is  $\alpha Th_t$  ( $\alpha > 1$ ) if all  $I$  players contribute  $Th_t$ , and zero otherwise. So the payoff of player  $i$  in period  $t$  is as follows:

$$Earning_{i,t} = \begin{cases} E + (\alpha - 1)Th_t, & \text{if } A_{i,t} = C \text{ and } A_{j,t} = C, \forall j \neq i \\ E, & \text{if } A_{i,t} = D \\ E - Th_t, & \text{if } A_{i,t} = C \text{ and } \exists j \neq i, \text{ s.t. } A_{j,t} = D \end{cases}$$

Players do not know others' actions when they make decisions. After each period, they get some feedback of the coordination result.

I adopt level- $k$  thinking in this model: level-0 players are nonstrategic (the specific definition of level-0 players may vary across games), while level-1 players best respond to level-0 players, and level-2 players best respond to level-1 players; more generally, level- $k$  players best respond to level- $k-1$  players for any  $k \geq 1$  (e.g., Costa-Gomes et al., 2001; Costa-Gomes & Crawford, 2006; Costa-Gomes et al., 2009). In a level- $k$  player's mind all opponents are level- $k-1$  players: for example, in a level-1 player's mind all opponents are level-0 players; in a level-2 player's mind all opponents are level-1 players.<sup>29</sup> The literature shows that most players are level-1 or level-2 thinking, and the ratio of level-0 players is small. So I assume there are only level-0, level-1 or level-2 players. But the theoretical results also apply when there are level- $k$  players ( $k \geq 3$ ).

In this model I assume that a level-0 player has a constant "willingness-to-give," which is the amount of effort she would like to contribute if there is no binary constraint for the decision in each period. This assumption is a natural extension from the case of continuous play because the continuous "willingness-to-give" can be considered as a "latent action" of the discrete binary decision in each period.<sup>30</sup> So if her willingness-to-give is larger than or

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<sup>29</sup> In cognitive hierarchy models (e.g., Camerer et al., 2003), level-2 players best respond not to level-1 players alone but to a mixture of level-0 and level-1 players. But this difference will not affect the theoretical results (Propositions 2.1-2.3).

<sup>30</sup> Interviews with players after experiments confirm that some players do adopt such a rule at least in the first period. For example, they say that they will give as long as the stake is below 10 (or 8, 12, etc.) points.

equal to the stake (then I call her a “high type”), she will give; otherwise she will not give (then I call her a “low type”). Note that since the stake may change, the type only applies for a certain period: a high type for a certain stake may not be a high type for a higher stake; vice versa. I further introduce a noise on level-0 players’ actions: a high type has a probability of  $\varepsilon$  ( $0 < \varepsilon < 1$ ) that she does not contribute (due to “mistakes”),<sup>31</sup> and a probability of  $1 - \varepsilon$  that she contributes; a low type has a probability of  $\eta$  ( $0 < \eta < 1$ ) that she contributes (due to “mistakes”), and a probability of  $1 - \eta$  that she does not contribute. It is natural to assume  $1 - \varepsilon > \eta$ , i.e., the high type is more likely to contribute than the low type. Level-1 players know (or believe) this decision rule of level-0 players; level-2 players know that level-1 players know this rule, etc.

A level-1 player has a prior belief about each level-0 player’s “willingness-to-give,” an unknown constant for her, chooses the best response according to her belief, and updates her belief according to the outcome and feedback of each period. Similarly, a level-2 player has a prior belief about level-1 players’ actions, chooses the best response according to her belief, and updates her belief after each period.

Suppose all  $I$  players are risk averse. A level-1 player  $i$ ’s belief about a level-0 player  $j$ ’s willingness-to-give is  $B_j^i$ , which follows a cumulative distribution function  $F_{j,t}^i()$ ,  $j \neq i$ .

A level-1 or level-2 player (or any level- $k$  player for  $k \geq 1$ )  $i$  will contribute in period  $t$  if and only if she believes the probability that all her opponents (in a level-1 player’s mind all opponents are level-0 players; in a level-2 player’s mind all opponents are level-1

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<sup>31</sup> This setup is similar to the study by Fudenberg et al. (2011) on the experimental play of repeated prisoners’ dilemma when intended actions are implemented with exogenous noises. But in this model only level-0 players may make mistakes, and level-1 players believe her opponents (in their minds all opponents are level-0 players) may make mistakes. As in their study, by believing that others may make mistakes, some cooperative players play “leniently”: they may not retaliate for the first defection of others.

players) will contribute exceeds a certain value  $\theta_i(Th_t)$ , which denotes her risk attitude. I call  $\theta_i(Th_t)$  the “reserved probability of success.” Under the assumption of risk aversion ( $U'(x) > 0$ ,  $U''(x) < 0$ ), it is easy to have the following lemma:

**Lemma 2.1.** The reserved probability of success is between 0 and 1; the higher the stake, the higher the reserved probability of success ( $0 < \theta_i(Th_t) < 1$ , and  $\partial \theta_i(Th_t) / \partial Th_t > 0$ ).

**Proof:** See Appendix A.3.

The intuition of Lemma 2.1 is simple: the higher the stake, the higher the “reserved probability of success” which makes a risk-averse “rational” player (e.g., a level-1 or level-2 player, but not level-0 players) willing to have a try.

Under the assumption of independent distributions on  $i$ ’s belief about all her opponents’ types, a level-1 player  $i$  will contribute at  $t$  if and only if:

$$\prod_{j \neq i} [(1 - \varepsilon)s_{j,t}^i + \eta(1 - s_{j,t}^i)] \geq \theta_i(Th_t) \quad (2.1)$$

Where  $s_{j,t}^i = \text{Prob}(B_j^i \geq Th_t) = 1 - F_{j,t}^i(Th_t)$  is  $i$ ’s belief regarding the probability that a level-0 player  $j$  is a high type in period  $t$ .

**Proposition 2.1.** The lower the  $Th_1$ , the higher the probability that the coordination at  $t=1$  will succeed.

**Proof:** See Appendix A.4.

Proposition 2.1 suggests that coordination will be more likely to succeed at the beginning with a lower stake level.

Next, I show the manner in which a level-1 player  $i$  updates her belief.

After  $t$ , if  $i$  observes her opponent  $j$  (in  $i$ 's mind,  $j$  is a level-0 player) does not give at  $t$  (with the information structure of both weakest-link experiments in this study and other coordination experiments,  $i$  may not necessarily observe the actions of  $j$  after each period), then according to the Bayes' rule, her posterior belief about the probability that  $j$  is a high type is:

$$h_{j,t}^i = \text{Pr ob}(B_j^i \geq Th_t \mid A_{j,t} = D) = s_{j,t}^i \varepsilon / [s_{j,t}^i \varepsilon + (1 - s_{j,t}^i)(1 - \eta)]$$

So at  $t+1$ , if the stake is still  $Th_t$ , then  $i$  believes that the probability that  $j$  will contribute is  $h_{j,t}^i(1 - \varepsilon) + (1 - h_{j,t}^i)\eta$ .

If  $i$  observes  $j$  gives at  $t$ , then her posterior belief about the probability that  $j$  is a high type is:

$$k_{j,t}^i = \text{Pr ob}(B_j^i \geq Th_t \mid A_{j,t} = C) = s_{j,t}^i(1 - \varepsilon) / [s_{j,t}^i(1 - \varepsilon) + (1 - s_{j,t}^i)\eta]$$

So at  $t+1$ , if the stake is still  $Th_t$ , then  $i$  believes that the probability that  $j$  will contribute is  $k_{j,t}^i(1 - \varepsilon) + (1 - k_{j,t}^i)\eta$ .

It is easy to show that  $k_{j,t}^i \geq h_{j,t}^i$  and  $k_{j,t}^i(1 - \varepsilon) + (1 - k_{j,t}^i)\eta \geq h_{j,t}^i(1 - \varepsilon) + (1 - h_{j,t}^i)\eta$ , given  $1 - \varepsilon > \eta$ . The intuition is as follows: if the high type has a higher probability to contribute than the low type, then the probability that  $j$  is a high type (and the probability that  $j$  will contribute next period facing the same stake) is larger when  $j$  contributes than when  $j$  does not.



Then, assuming independent distributions of all  $j$ 's actions at  $t+1$ ,  $i$  believes that the probability that all  $j$  will contribute at  $t+1$  with the same stake ( $Th_{t+1}=Th_t$ ) is  $\prod_{j \neq i} \{[h_{j,t}^i 1(A_{j,t}=D) + k_{j,t}^i 1(A_{j,t}=C)](1-\varepsilon-\eta) + \eta\}$ , where  $1(\bullet)$  equals one if the argument in the parenthesis is true, and zero otherwise. Assuming that  $i$  observes that the number of contributing opponents (in  $i$ 's mind all opponents are level-0 players) at  $t$  is  $m$  ( $0 \leq m \leq I-1$ ), since  $k_{j,t}^i \geq h_{j,t}^i$  and  $1-\varepsilon > \eta$ , the larger the  $m$ , the larger the probability  $i$  believes that all opponents will contribute at  $t+1$  when  $Th_{t+1}=Th_t$ , thus the larger the probability  $i$  will contribute at  $t+1$ .

Since a level-2 player  $k$  knows the rules of decision and belief updating for level-1 players, the larger the  $m$ , the larger the probability  $k$  believes that all  $i$  will contribute at  $t+1$  when  $Th_{t+1}=Th_t$ , thus the larger the probability  $k$  will contribute at  $t+1$ .<sup>32</sup>

For a level-0 player  $j$ , no matter how large the  $m$  is, the probability that she will give is always  $(1-\varepsilon)1(B_j \geq Th_t) + \eta 1(B_j < Th_t)$ .

So when players are informed about the number ( $m$ ) of contributors at  $t$  with a stake  $Th_t$ , the larger the  $m$ , the higher the probability that all level-0, level-1 and level-2 players will contribute at  $t+1$  if  $Th_{t+1}=Th_t$ , so the higher the probability that the coordination at  $t+1$  will succeed if  $Th_{t+1}=Th_t$ . Thus we have:

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<sup>32</sup> Similarly, a level-3 player knows the strategy rule of level-2 players, so the larger the  $m$ , the larger the probability that a level-3 player will contribute at  $t+1$ , and so on.

**Proposition 2.2** When players are informed about the number ( $m$ ) of contributors at  $t$  with a stake  $Th_t$ , the larger the  $m$ , the higher the probability that the coordination at  $t+1$  will succeed if  $Th_{t+1} = Th_t$ .

**Proof:** See above.

Proposition 2.2 shows the importance of information feedback in belief updating. When players are informed about the exact number of contributors after each period, they can update their beliefs more efficiently. If they know most members have contributed and allow a probability that the non-contributor's real intention is to contribute (i.e., the non-contributor makes a mistake at playing), then they are more likely to keep contributing at the same stake even coordination has failed, thus may become successful later. However, if they fail in coordination and do not know most members have contributed, or they know most members have not contributed, they are less likely to become successful.

Below I show that a slow increase in stake is always no worse than a quick increase for coordination.

**Proposition 2.3.** No matter whether a group succeeds or fails at  $t$  with a stake  $Th_t$ , the lower the  $Th_{t+1} (\geq Th_t)$ , the (weakly) higher the probability that the coordination at  $t+1$  will succeed.

**Proof:** See the proof of Proposition 2.1 and replace  $t=1$  with  $t = t + 1$ .

In Section 2.5 I further discuss the theoretical explanations.

### 2.2.2 The Case When Free Riding Is Possible

Belief is also important in coordination when free riding is possible. A self-interested player will contribute if and only if she believes that her contribution will be pivotal, which is hard for her to decide.<sup>33</sup> Even the player is conditionally cooperative (Fischbacher et al., 2001), i.e., she is willing to cooperate if others cooperate as well or if she believes others will cooperate, she also needs to have a clear belief about the probability distribution of the number of contributors in each period.

Although the model in Section 2.2.1 suggests a potential advantage of gradualism in weakest-link coordination, it cannot be taken for granted when free riding is possible because the belief updating process under this case is less consistent and much more difficult. Theoretically, when free riding is possible in the coordination game (or “discrete public good game”), there are many pure equilibria in the one-shot game (for example, in the third experiment in this study, when three out of four members are required to contribute, there are five pure equilibria in the one-shot game), which is much more complicated than the binary weakest-link games (in which there are only two pure equilibria in the one-shot game: either all contribute, or none contributes). Even a group has succeeded in coordination (i.e., at least the required number of members contribute) and even gradualism makes coordination easier at the beginning with a low stake, in the next period some group members may want to free ride or expect others to free ride, so one’s belief about others’ actions may be much less consistent across periods than in the weakest-link

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<sup>33</sup> More accurately, a self-interested player will contribute if and only if the gain in expected utility from contributing is non-negative. Given the payoff matrix in the third experiment (see Section 2.6), let  $P_3$ ,  $P_2$ ,  $P_1$  and  $P_0$  denote  $i$ ’s belief about the probability that exactly three, two, one and zero other group member(s) will contribute,  $i$  will contribute if and only if the following long inequality holds:  $P_2[U_i(E + Th_i) - U(E)] \geq P_3[U_i(E + 2Th_i) - U_i(E + 1.25Th_i)] + (P_0 + P_1)[U_i(E) - U_i(E - Th_i)]$ .

case (in the latter, once all group members contribute, players have a high belief that all others will keep contributing at the same stake since deviation cannot bring personal benefits.) So it is more difficult for the players to update their beliefs about others' actions in each period, and coordinating on certain outcomes (especially the high-efficiency equilibria in which exactly the required number of members contribute, and the non-equilibrium outcomes when more than enough members contribute) is difficult: there are supposed to be more fluctuations over periods.

The above discussion suggests that the role of gradualism is unclear when free riding is possible. Thus I adopt an empirical method to answer this question in Section 2.6.

## **2.3 Experimental Designs**

### **2.3.1 Sample and Payoff Structure**

Table 2.1 lists the experimental designs of the three experiments. Although I have introduced the design of the first experiment in Chapter 1, here I repeat some aspects of the design for this chapter to be self-contained. I focus on the two weakest-link coordination experiments in this section and discuss the third experiment that allows for free riding in Section 2.6.

Table 2.1: Experimental Designs

Experiment	Experiment 1 (weakest-link with limited information)	Experiment 2 (weakest-link with richer information)	Experiment 3 (chance for free riding)
Location, time and No. of subjects	Renmin University, July 2010, N=256	Xiamen University, May 2011, N=216	Xiamen University, May 2011, N=144
Group size, required No. of contributors	4, 4	4, 4	4, 3
Information structure	1. Do not know No. of periods 2. Do not know future stakes 3. Only know whether successful or not	1. Know No. of periods 2. Know future stakes 3. Know how many members contribute	1. Know No. of periods 2. Know future stakes 3. Know how many members contribute
Treatments	Big Bang Semi-gradualism Gradualism High Show-up Fee	Big Bang Semi-gradualism Gradualism	Big Bang Semi-gradualism Gradualism
Method to eliminate income effect	Introduce the “High Show-up Fee” treatment	Randomly pick up one period for payment (multiplied by the No. of periods)	Randomly pick up one period for payment (multiplied by the No. of periods)
Conversion rate	1 <i>yuan</i> =40 points	1 <i>yuan</i> =30 points	1 <i>yuan</i> =30 points

Experiment 1 (limited-information weakest-link game), as introduced in Chapter 1, was conducted at Renmin University of China in Beijing, China in July 2010 with 256 subjects recruited via the Bulletin Board System (BBS) and posters at the university. Experiment 2 (rich-information weakest-link game) was conducted at the Finance and Economics Experimental Lab at Xiamen University in Xiamen, China in May 2011 with 216 subjects recruited via the Online Recruitment System for Economic Experiments (ORSEE) at the university. All laboratory sessions were computerized using the z-Tree experiment software package (Fischbacher, 2007). Both the instructions and the information shown on the computer screen were in Chinese. The instruction handouts (see Appendix A.1) were read aloud for everyone in the same session to hear. Each session lasted between 30 minutes and one hour (including the time for signing up, reading instructions, a quiz to guarantee the accurate understanding of the experimental rules, and final payments.).

Table 2.2 shows the demographic characteristics of subjects in the three experiments. Subjects' characteristics in experiment 2 are similar to those in experiment 1, except that in experiment 2 the ratio of students is eight percentage points higher (99% versus 91%), and the distribution of concentration is not identical (e.g., the ratios of economics, other social sciences, humanities and science majors are seven percentage points higher, seven percentage points lower, eleven percentage points lower, and six percentage points higher in experiment 2, respectively).<sup>34</sup> Subjects' characteristics in experiment 3 are very similar to those in experiment 2, except that the ratio of economics majors is five percentages lower, while that of humanity majors are five percentage points higher.

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<sup>34</sup> There may be a concern that economics and business students play differently with other students. I address this concern by examining actions in the first period, as well as actions in the second period conditional on the coordination outcome of the first period, and find no differences between economics/business students and other students. Results are available upon request.

Table 2.2: Demographic Characteristics of Subjects for All Three Experiments

Variable	Mean and standard deviation			Observations		
	Experiment 1	Experiment 2	Experiment 3	Experiment 1	Experiment 2	Experiment 3
Age	22.05 (3.25)	22.72 (1.99)	22.13 (1.69)	216	255	144
Male	0.41 (0.49)	0.44 (0.50)	0.40 (0.49)	216	255	144
Income	1.32 (1.38)	1.30 (1.05)	1.40 (1.17)	215	255	144
Family Income	5.63 (2.69)	5.82 (2.65)	5.88 (2.85)	200	189	141
Family Economic Status	2.60 (0.74)	2.65 (0.74)	2.57 (0.78)	215	254	144
Risk Aversion Index	4.47 (1.80)	4.57 (1.86)	4.51 (1.67)	215	250	142
Han nationality	0.91 (0.29)	0.95 (0.21)	0.92 (0.28)	216	255	144
Student	0.91 (0.29)	0.99 (0.10)	0.98 (0.14)	216	255	144
<b>Concentration:</b>						
Economics	0.12 (0.33)	0.19 (0.39)	0.14 (0.35)	216	241	144
Other Social Sciences	0.16 (0.37)	0.09 (0.28)	0.06 (0.23)	216	241	144
Business	0.27 (0.45)	0.27 (0.45)	0.29 (0.46)	216	241	144
Humanity	0.12 (0.33)	0.01 (0.10)	0.06 (0.24)	216	241	144
Science	0.15 (0.35)	0.21 (0.41)	0.23 (0.42)	216	241	144
Engineering	0.17 (0.38)	0.16 (0.36)	0.17 (0.38)	216	241	144
Medical/Health	0.01 (0.09)	0.04 (0.39)	0.03 (0.18)	216	241	144

Note: Statistics regarding experiment 1 is adopted from Chapter 1. Income is a scale variable from 0 to 13, with a higher value indicating a higher income (0: no income; 1: annual income < 5000 yuan; 13: annual income > 160,000 yuan). Family income is a scale variable from 1 to 12, with a higher value indicating a higher income (1: annual income < 5000 yuan; 12: annual income > 200,000 yuan). Family economic status is coded in the following way: 1 (lower), 2 (lower middle), 3 (middle), 4 (upper middle), 5 (upper). Risk aversion index is a scale from 0 to 10, with a higher value approximately indicating higher risk aversion, and is measured as the number of lottery A chosen by the subject in the questionnaire (see Appendix A.2).

Experiment 1 included 18 sessions (each had 12 or 16 subjects); experiment 2 included 9 sessions (each had 24 subjects). In each session, I randomly assigned subjects to groups of four. So the whole sample consisted of 64 groups for experiment 1 and 54 groups for experiment 2. As discussed in Chapter 1, each experiment included two stages: the first stage comprised 12 periods, while the second one had eight periods. Group compositions were fixed in the first stage; but when subjects entered the second stage of the game, they were randomly reshuffled into new groups of four, and new group members might not necessarily come from the same treatment as in the first stage; this rule is made common knowledge. In the second stage, group compositions were fixed, and stakes were all set at the highest stake for all periods and all groups, i.e., those treated in different treatments in the first stage faced the same stake in each period of the second stage.

In this chapter I only present the results in the first stage.<sup>35</sup>

In each period, I endowed subjects with 20 points and asked them to give a certain number of points (which I refer to as stake) to their assigned groups' common pools. This stake could vary across periods, and each subject could only choose either not to give or to give exactly the stake. If all four members in a group gave, then each member not only got back the points she had given, but also gained an extra return equaling the stake; otherwise each member finished each period only with her points remaining, and the points she gave out were wasted. I avoided the use of terms with strong connotations (e.g., I use the natural term "give" rather than "contribute" in the instruction). Although a concrete context like a fictitious corporate or project may make the instruction easier to understand, I used the abstract terminology for two reasons. First, it was to avoid any unwanted framing or

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<sup>35</sup> The purpose of introducing the second stage in experiment 1 is to explore a potential externality of coordination building (or collapse) across various social groups (see Chapter 1). To make the results comparable, I also have a second stage for experiments 2 and 3.



experimenter demand effects<sup>36</sup> (for a fair comparison, all three experiments adopted the same abstract way.) Second, the binary choice set in each period was easy to understand, and the examples and quiz guaranteed the accurate understanding.

In sum, the following formula (in the instruction, the payoff structure is presented in texts, which is easier for subjects to understand; see Appendix A.1) describes individual earnings in each period:

$$Earning_{i,t} = \begin{cases} 20 + Th_t, & \text{if } A_{i,t} = C \text{ and } A_{j,t} = C, \forall j \neq i \\ 20, & \text{if } A_{i,t} = D \\ 20 - Th_t, & \text{if } A_{i,t} = C \text{ and } \exists j \neq i, \text{ s.t. } A_{j,t} = D \end{cases}$$

where  $Earning_{i,t}$  is  $i$ 's earning in period  $t$ ,  $Th_t$  is the stake at  $t$ ,  $A_{i,t}$  and  $A_{j,t}$  are the actions of  $i$  and  $j$  at  $t$ , respectively ( $i$  and  $j$  are in the same group),  $C$  represents “cooperate” (“give”), and  $D$  represents “deviate” (“not give”).

The final payment was the performance payment plus a show-up fee of 10 *yuan*.<sup>37</sup> For experiment 1, the performance payment was the total earning accumulated over periods. For experiment 2, to deal with the concern of an income effect in the laboratory (see Section 2.4 for details), the program randomly picked up one period's earning of the first stage (multiplied by the number of periods in the first stage) as the performance payment for that stage; the performance payment in the second stage is the total earning for all periods in that stage, as in experiment 1.

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<sup>36</sup> Some studies (e.g., Brandts & Cooper, 2006a) adopt concrete contexts. Cooper and Kagel (2003) find that the use of meaningful context speeds up strategic plays.

<sup>37</sup> For the “High Show-up Fee” treatment (see the description in Section 2.4) in experiment 1, the show-up fee was 12 *yuan*. In this chapter, I will only focus on the three main treatments. The show-up fees in experiments 1 and 2 were denominated in point and *yuan*, respectively.

The conversion rate for experiment 1 was 40 points per *yuan*, and that for experiment 2 was 30 points per *yuan*. The more generous conversion rate in experiment 2 was considered necessary to insure adequate subjects in the new location where experimental payments were generally higher.

The average payments (including the show-up fees) for experiments 1 and 2 were 21.4 and 25.9 *yuan* (about 3~4 dollars),<sup>38</sup> respectively. These earnings could afford ordinary meals at student canteens for one to two days, and were sufficiently large to generate a good supply of subjects. Regarding the purchasing power, it is comparable to experimental payments (for one-hour experiments) in other countries.

### 2.3.2 Treatment Group Assignments

Each experiment consisted of three main treatments: “Big Bang,” “Semi-gradualism” and “Gradualism.” All groups in the three main treatments face the same stake in the second half of the first stage (periods 7-12); but the stake paths differ in the first half (periods 1-6). The stakes over 12 periods are shown in Figure 2.1: for the “Big Bang” treatment, the stakes are always at the highest level, which is 14; for the “Semi-gradualism” treatment, they are two for the first six periods and set at the highest stake for the next six periods; for the “Gradualism” treatment, they increase from two to 12 with a step of two for the first six periods and fix at the highest stake for the next six periods.

Table 2.3 shows the randomizations in all three experiments (one panel for each experiment) worked very well in assigning subjects into the treatments. The default treatment is “Gradualism,” so the constant shows the means for the “Gradualism” treatment,

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<sup>38</sup> The exchange rate when the experiments were conducted was about 1 USD=6.7 *yuan* and 1USD=6.5 *yuan* for experiments 1 and 2, respectively.

and the coefficients of “Big Bang” and “Semi-gradualism” show the differences between those two treatments and the “Gradualism” treatment. In each experiment, there are few variables the values of which are statistically significantly across treatments.

**Table 2.3: Comparison of Subjects' Characteristics by Treatment for Each Experiment**

	Dependent Variable								
	Age	Male	Income	Family Economic Status	Risk Aversion Index	Han Nationality	Student	Economics Major	Business Major
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
<b>Experiment 1 (Limited-information Weakest-link)</b>									
Big Bang	-0.306 (0.612)	-0.042 (0.081)	-0.125 (0.241)	-0.083 (0.123)	0.474* (0.279)	0.028 (0.046)	0.083* (0.046)	0.026 (0.056)	0.006 (0.078)
Semi-gradualism	-0.278 (0.627)	0.076 (0.083)	-0.360 (0.236)	0.217* (0.128)	0.424 (0.295)	0.013 (0.048)	0.055 (0.050)	-0.000 (0.053)	0.030 (0.080)
Constant	22.236***	0.389***	1.444***	2.597***	4.097***	0.903***	0.875***	0.104***	0.284***
Observations	255	255	255	254	250	255	255	241	241
R-squared	0.00	0.01	0.01	0.04	0.02	0.00	0.03	0.00	0.01
<b>Experiment 2 (Richer-information Weakest-link)</b>									
Big Bang	-0.389 (0.347)	-0.167** (0.082)	0.005 (0.193)	-0.311** (0.122)	-0.146 (0.327)	-0.056 (0.038)	0.028 (0.020)	-0.014 (0.066)	0.097 (0.074)
Semi-gradualism	-0.653* (0.367)	-0.070 (0.084)	-0.194 (0.159)	-0.403*** (0.121)	-0.028 (0.306)	0.000 (0.028)	0.028 (0.020)	-0.000 (0.066)	0.056 (0.073)
Constant	23.07***	0.514***	1.361***	2.889***	4.625***	0.972***	0.972***	0.194***	0.222***
Observations	216	216	215	215	215	216	216	216	216
R-squared	0.018	0.019	0.008	0.054	0.001	0.016	0.019	0.000	0.008
<b>Experiment 3 (Allowing for Free Riding)</b>									
Big Bang	0.184 (0.342)	0.025 (0.102)	0.409 (0.266)	0.085 (0.159)	-0.307 (0.357)	-0.016 (0.067)	-0.020 (0.036)	-0.092 (0.074)	-0.057 (0.099)
Semi-gradualism	-0.023 (0.360)	-0.008 (0.102)	0.230 (0.185)	0.224 (0.153)	-0.286 (0.340)	0.087* (0.051)	0.022 (0.022)	-0.089 (0.075)	-0.178* (0.092)
Constant	22.09***	0.391***	1.196***	2.457***	4.711***	0.891***	0.978***	0.196***	0.370***
Observations	141	141	141	141	139	141	141	141	141
R-squared	0.003	0.001	0.020	0.014	0.007	0.027	0.014	0.015	0.027

Note: Panel 1 is adopted from Chapter 1. Robust standard errors in parentheses. \*, \*\*, \*\*\* significant at 10%, 5% and 1% respectively.

Since the stake paths for these treatments differ in periods 1-6, it may have imposed an income effect in the laboratory. To estimate how much of the difference in performances among the three main treatments in periods 7-12 is the real treatment effect rather than an income effect, I adopted two different methods in experiments 1 and 2, respectively. For experiment 1, I introduced the “High Show-up Fee” treatment to capture the potential income effect, which is identical to the “Big Bang” treatment except for a higher show-up fee (see Chapter 1 for more details). For experiment 2, I adopted a popular way in the experimental literature to eliminate the income effect: randomly picking up one period in the first stage, and then using the earning of that period (multiplied by the number of periods in the first stage) as the performance payment for the first stage.<sup>39</sup> Moreover, in both experiments, I randomized groups into various treatments, thus eliminating the potential effect of subjects’ incomes in the real world.

The information structure was as follows.

Regarding the information structure, the information feedback in experiment 1 was limited (after each period, subjects only knew whether the project was successful or not, i.e., whether all four group members including herself contributed), while that in experiment 2 was richer although still imperfect (after each period, subjects knew exactly how many group members contributed, but not exactly who contributed). In addition, in experiment 1,

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<sup>39</sup> Both methods have their advantages and disadvantages. The concern about the popular method of random payments is that it may make some subjects less serious at playing in each period; but if this undesired effect is not correlated with the treatments or if others play even more seriously (because the earning in the selected period will be multiplied), on average it would be less a concern. On the other hand, the calibration of the show-up fee difference between the “High Show-up Fee” treatment and other treatments is difficult, although the parameter in experiment 1 turns out to be fine; moreover, the income effect may be nonlinear, so it is hard to extrapolate the effect of a specific income difference. Thus in experiments 2 and 3, I switch to the more popular way, although it is imperfect.

subjects did not know the exact number of periods and the stakes of future periods beforehand; in experiment 2, subjects knew these.<sup>40</sup>

Communication across players was not allowed. I chose this design for two reasons. First, communication is often impossible or ineffective in the real world. Second, this design makes coordination more difficult.<sup>41</sup>

At the end of the experiments, I asked subjects to complete a brief survey. The survey collected information on age, gender, nationality, educational level, concentration at school, working status and income, in addition to eliciting risk preferences over lotteries (the questions on risk preferences were adopted from Holt & Laury, 2002, but in a different currency unit; see Appendix A.2).

## 2.4 Results

In this section I present the findings in summary tables, figures and regressions. I analyze the effect of treatments on the following four outcome variables per period: whether a group coordinates successfully, the number of group members contributing, whether an individual contributes, and the individual's earning. I compare the results of experiments 1 and 2 (see Chapter 1 for more detailed results of experiment 1).

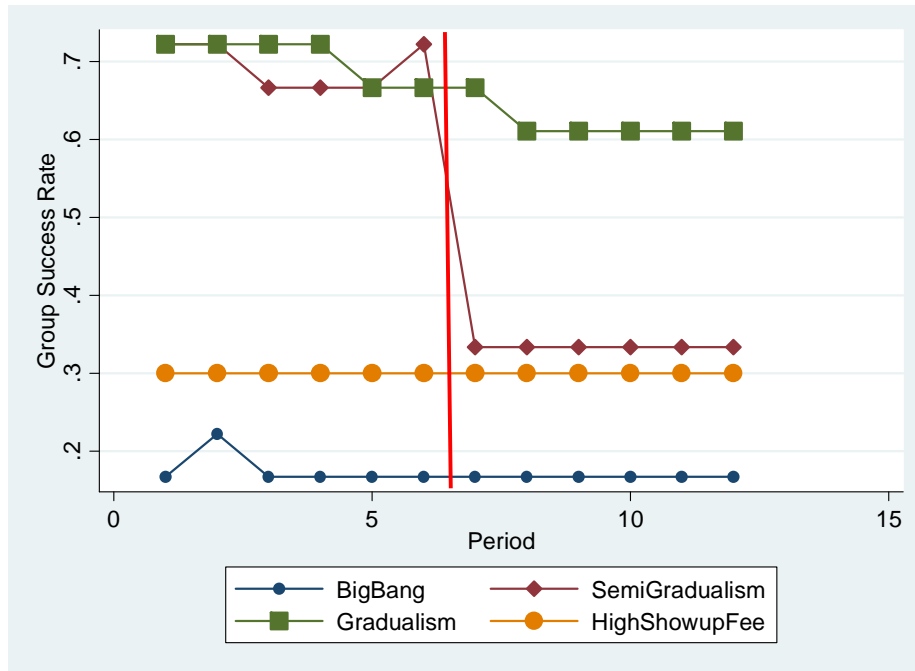
**Result 1: in period 1, the contribution and success rates are higher for groups with a lower stake in both experiments; but there are no obvious differences regarding contribution rates in period 1 between experiments 1 and 2.**

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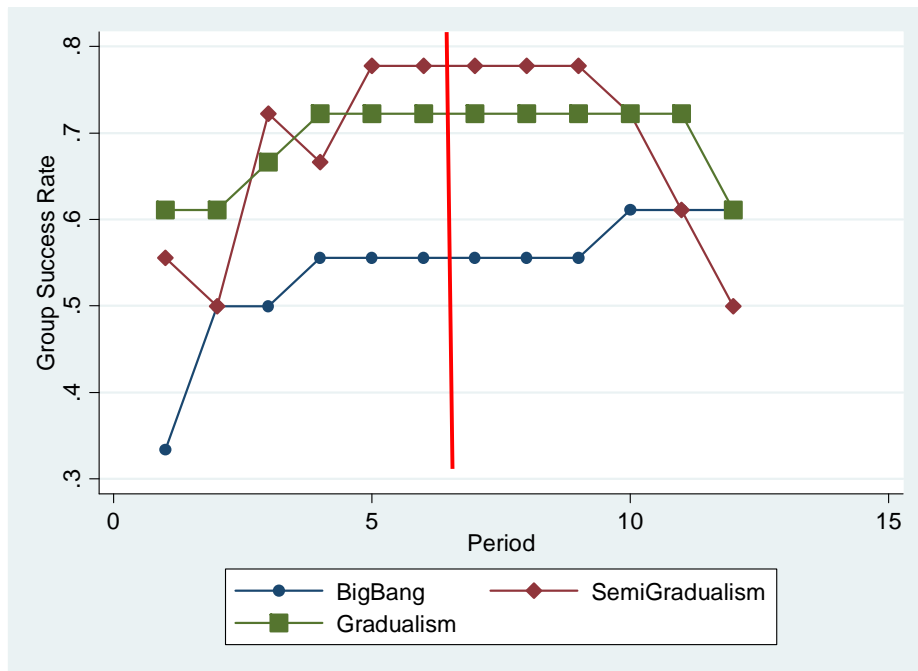
<sup>40</sup> In some cases of the real world, people do not know the exact number of coordination opportunities and the levels of future interactions beforehand; in other cases, they know.

<sup>41</sup> Ostrom (2010) summarizes that communication improves cooperation. Charness (2000) shows that communication helps coordination in small groups, while Weber et al. (2001) and Chaudhuri et al. (2009) find that large group coordination is still difficult even with communication.

Figure 2.2 shows the success rates over periods. In period 1, for experiment 1, over two thirds of “Semi-gradualism” and “Gradualism” groups coordinate successfully at the low stake, while only 16.7% (or 30%) of the “Big Bang” (or “High Show-up Fee”) groups are successful at the high stake. For experiment 2, about 60% of “Semi-gradualism” and “Gradualism” groups coordinate successfully at the low stake, while only about 30% of the “Big Bang” groups are successful at the high stake. This result clearly confirms the prediction of Proposition 2.1.



A. Experiment 1 (Limited-information Weakest-link; adopted from Chapter 1)



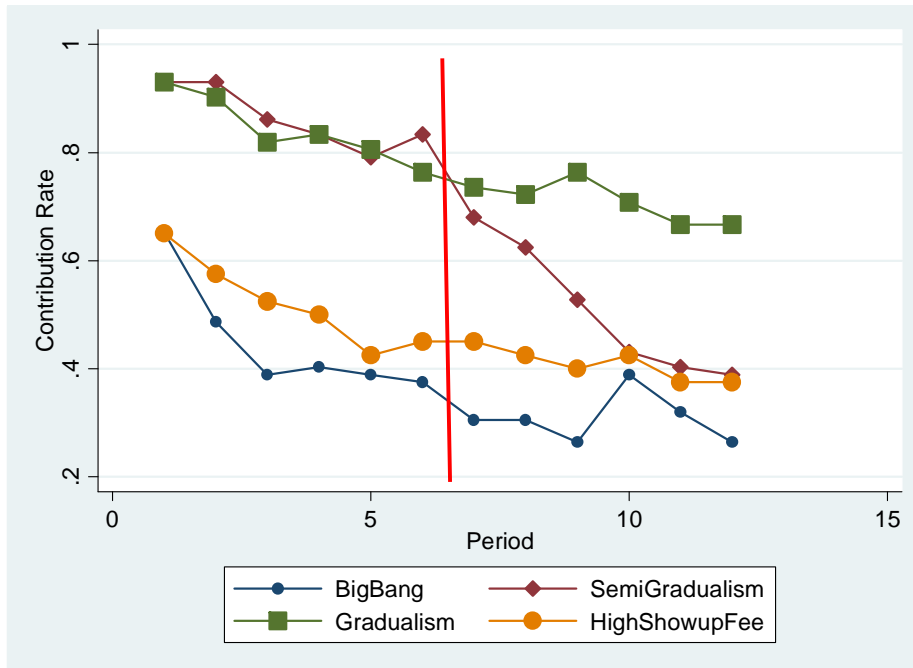
B. Experiment 2 (Richer-information Weakest-link)

A group is successful in coordination if all four members give the stake in that period.  
 Figure 2.2: Success Rates of Groups by Treatment and Period in Weakest Link Experiments

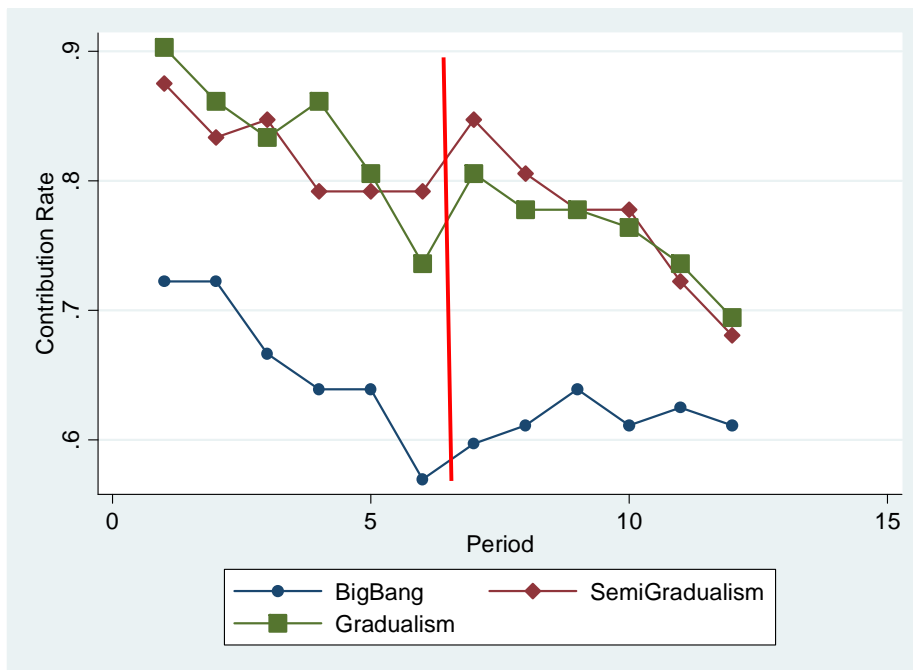


Figure 2.3 shows the contribution rates over periods. Regarding the contribution rates, the differences are also obvious. The contribution rates in experiment 1 are about 90% for “Semi-gradualism” and “Gradualism” groups, and below 70% for “Big Bang” groups. The numbers in experiment 2 are similar: about 90% for “Semi-gradualism” and “Gradualism” groups, and about 70% for the “Big Bang” groups. But the numbers are similar between experiments 1 and 2.

We can see the differences of success rates across treatments are much more obvious than those of contribution rates. This is because the efficiency in “Semi-gradualism” and “Gradualism” groups is much higher with a low stake: fewer subjects waste their contributions.



A. Experiment 1 (Limited-information Weakest-link; adopted from Chapter 1)



B. Experiment 2 (Richer-information Weakest-link)

Figure 2.3: Contribution Rates by Treatment and Period in Weakest Link Experiments

**Result 2: in experiment 1, the success rates in the first six periods change little over periods; in experiment 2, the success rates increase over the first six periods, especially for “Big Bang” and “Semi-gradualism” groups in which stakes are constant for these periods.**

Although the contribution rates for all treatments decline steadily over periods, the success rates do not. This shows that contribution rates decline mainly because those contributing in the failed groups start giving up when they find coordination has failed.

Interestingly, for experiment 2 which adopts a richer information structure, success rates can even increase even as contribution rates declines, especially for “Big Bang” and “Semi-gradualism” groups. This result can be seen from Figure 2.2. Below (in result 4) I will further explore why success rates can increase over periods.

**Result 3: in experiment 1, the success rate of the “Semi-gradualism” treatment decreases sharply from period 6 to period 7 (from about 70% to only 33.3%), while that of the “Gradualism” treatment does not; in experiment 2, both do not.**

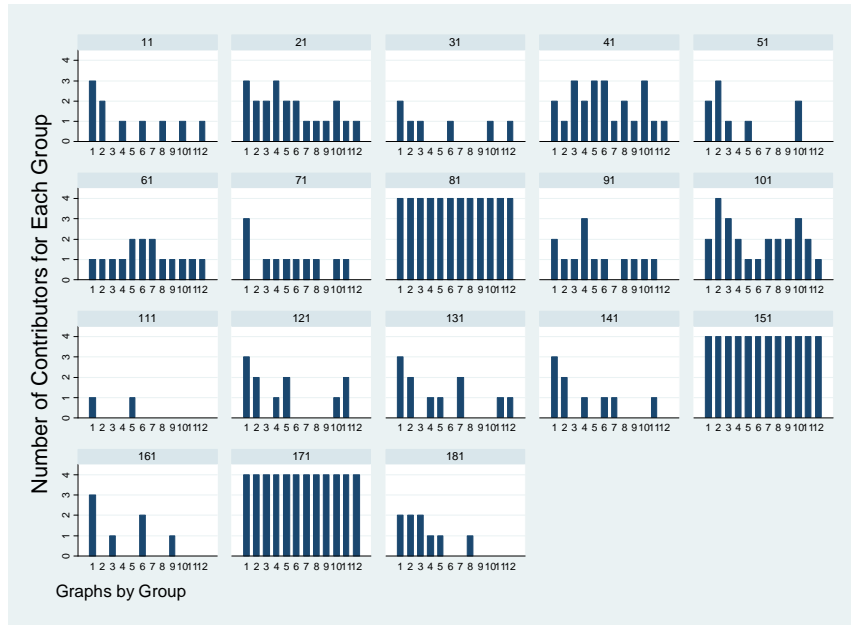
This is clearly shown in Figure 2.2. Thus the “Gradualism” treatment has a higher success rate in the high-stake periods than the “Semi-gradualism” treatment in experiment 1; but the difference is not obvious in experiment 2.

In Figure 2.4 I examine how coordination results vary across periods for each group by each treatment. For each group, the horizontal axis indicates the period, and the vertical axis indicates the number of contributors in that group: a bar with value one indicates successful coordination, while that with value zero indicates failed coordination. Each group is identified by a code in the following way: the lowest digit indicates the treatment type

(1="Big Bang," 2=Semi-gradualism," 3="Gradualism," 4="High Show-up Fee"); the highest one or two digits indicate the session number (1-18). On the top of the graph for each group is the 2-digit or 3-digit code for that group, which is constructed in the following way: the lowest digit indicates the treatment type (1="Big Bang," 2="Semi-gradualism," 3="Gradualism"); the highest one or two digits indicate the session number (1-9).

A further look at Figure 2.4 regarding the number of contributors for each group shows that all successful "Semi-gradualism" groups in period 6 keep successful in period 7 in experiment 2. This result partially confirms the prediction of Proposition 2.3 that a gradual increase of stake is always no worse than a sudden increase, although the advantage of a gradual increase (versus a sudden increase) is not found in experiment 2.

In experiment 1, the contribution rate of the "Semi-gradualism" groups decreases about 15 percentage points from period 6 to period 7; but the success rate has a bigger (about 40 percentage points) decrease, because the decrease in contribution rate is almost completely due to the deviation of one or two group members in successful groups of period 6 (see Figure 2.4).



A: “Big Bang” Groups in Experiment 1 (Limited-information Weakest-link)

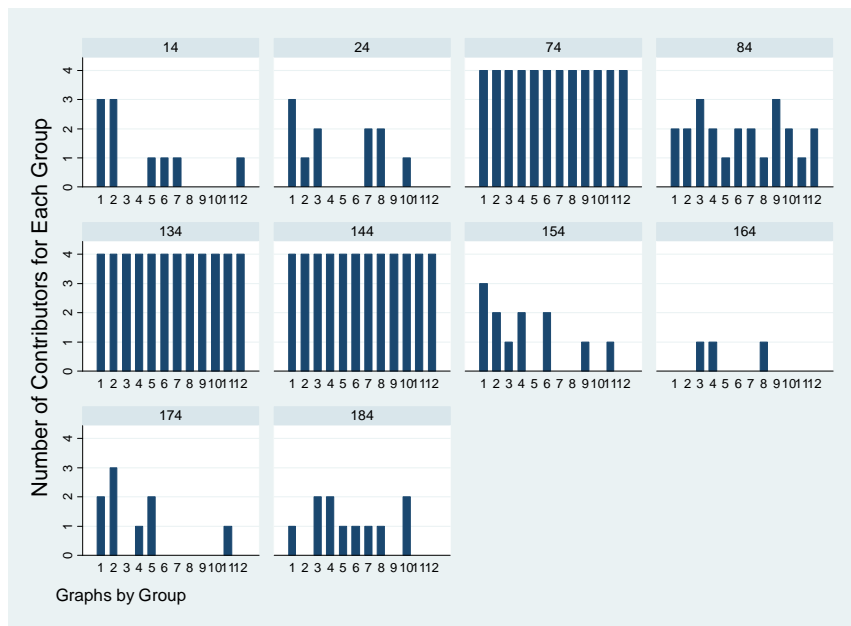


B: “Semi-gradualism” Groups in Experiment 1 (Limited-information Weakest-link)

Figure 2.4: Group Coordination Results for Each Treatment in Weakest Link Experiments

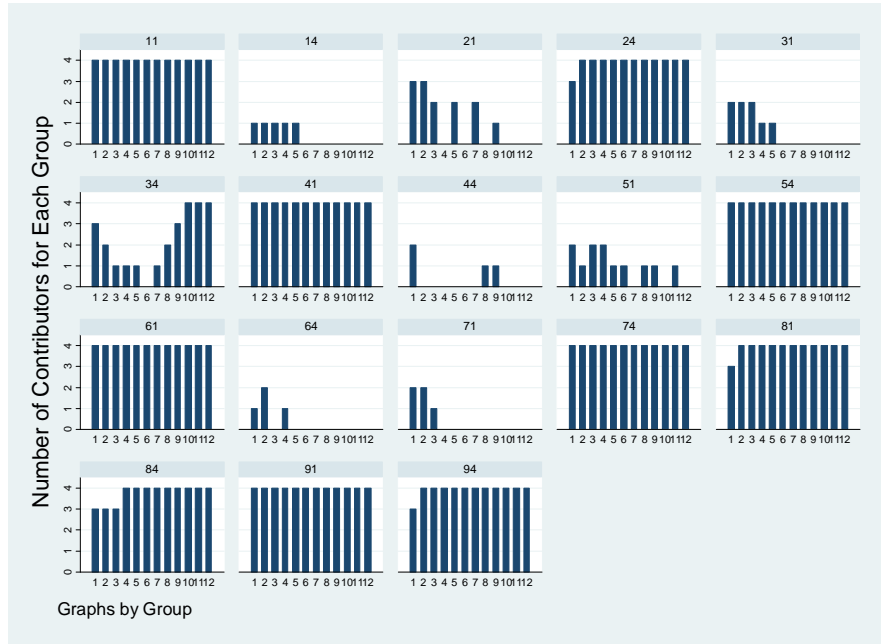


C: "Gradualism" Groups in Experiment 1 (Limited-information Weakest-link)

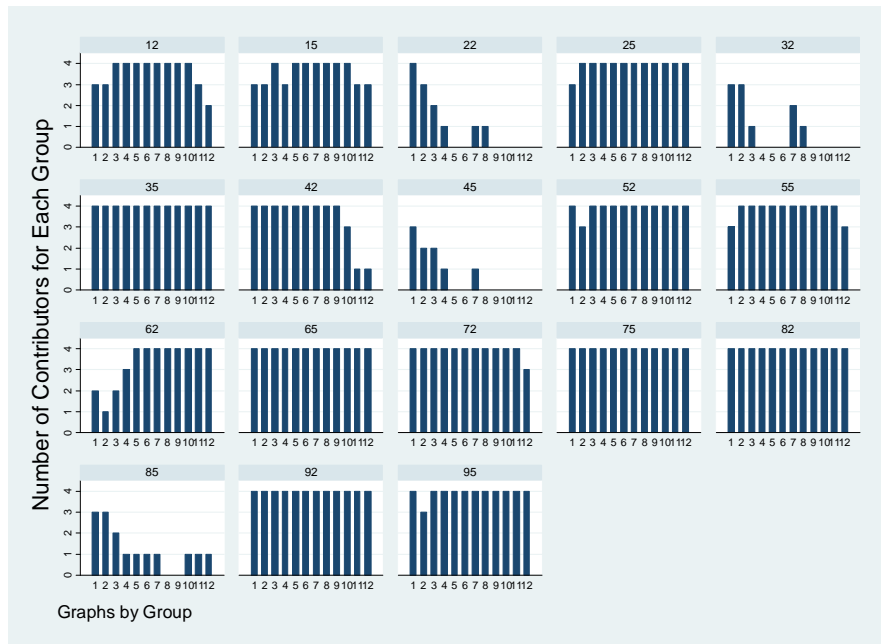


D: "High Show-up Fee" Groups in Experiment 1 (Limited-information Weakest-link)

Figure 2.4 (Continued)



E: “Big Bang” Groups in Experiment 2 (Richer-information Weakest-link)



F: “Semi-gradualism” Groups in Experiment 2 (Richer-information Weakest-link)

Figure 2.4 (Continued)



Panel G: “Gradualism” Groups in Experiment 2 (Richer-information Weakest-link)

Figure 2.4 (Continued)

This different result about period 7 between experiments 1 and 2 may be due to the experimental setting that the sharp increase in stake is foreseeable in experiment 2 but not in experiment 1 (see Table 2.1 for the experimental designs). When the shock to the stake is unanticipated, some subjects become unwilling to contribute even their groups have coordinated successfully with a much lower stake. Of course there might be another reason: players may be more willing to try contributing when they first face a stake level if they know they will be informed *ex post* the number of contributors. However, this does not seem to be a good explanation because there are no differences between the two experiments in the contribution rates in period 1: subjects in experiment 2 are unlikely to have more contributing trials just because they know that the *ex post* feedback will be richer.



**Result 4: in experiment 1, conditional on failed coordination, most groups fail with the same stake later; in experiment 2, when there are three out of four members contributing, more groups can switch from failed coordination to success at the same stake, but not for the “Gradualism” groups when the stake keeps increasing in the first six periods.**

We can see this from Figure 2.4. More specifically, in experiment 2 with richer information, there are six (seven) “Big Bang” (“Semi-gradualism”) groups with three contributors at the beginning, and five (four) of them are able to become successful. On the contrary, only two of the seven “Gradualism” groups with three contributors at the beginning can make it later. As a result, the difference between the success rates of “Big Bang” and “Gradualism” groups shrinks over time.

However, in experiment 1 with limited information, only one of 20 groups with three contributors at the beginning (seven “Big Bang,” five “Semi-gradualism,” five “Gradualism” and three “High Show-up Fee” groups) becomes successful later.

For those groups with less than three contributors, few of them can become successful later no matter whether the information feedback is limited or rich. In experiment 1 (or 2), only one (or one) of twelve (or seven) such groups succeeds subsequently.

This key result shows the advantage of information. When subjects are informed about the exact number of contributors and when they are close to success (i.e., three out of four contribute), they are more likely to become successful. On the contrary, if they do not know that they are close to success, they are much less likely to become successful. This is consistent with Proposition 2.2.

This result shows that gradualism helps in weakest-link coordination building by the channel of “information gathering” which facilitates belief updating. In experiment 1 with limited information, by having more groups successful at the beginning with a low stake and increasing the stake slowly, gradualism helps provide the key information for a successful group at the beginning that they are close to the final success with a high stake; for the “Big Bang” treatment, a high beginning stake harms coordination and since subjects in a failed group do not know how many members have contributed, they do not know how far they are from success, thus very difficult to become successful in the next period even they only need one more contributor to succeed. Such an “information gathering” function of gradualism can also be provided by the richer information structure, so in experiment 2 even “Big Bang” groups can attain information about how close they are to the final success, which facilitates coordination and makes success rates increase over time.

It is noteworthy that the information is still imperfect even in experiment 2: subjects only know how many members have contributed, but not exactly who and they do not know the decision history of a specific member. This is different from the studies by Berninghaus and Ehrhart (2001), and Brandts and Cooper (2006a) on the role of perfect *ex post* monitoring, and Deck and Nikiforakis (2010) on the effect of perfect real time monitoring in coordination.<sup>42</sup> This study shows that even a small piece of further information (the exact

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<sup>42</sup> These studies find a positive effect of perfect monitoring in coordination games. In Berninghaus and Ehrhart (2001), there are three treatments about the information feedback of a previous period: the group minimum effort, the distribution of group members’ choices, and each group member’s individual choice. The first and second treatment are similar to the limited information and the richer information cases in this chapter, respectively, except that in their experiment the entire history data are shown on the computer screen, while in this study only the result of the previous period is shown to subjects. Consistent with this study, they also find the richer (but still imperfect) information structure improves coordination, although their sample size is small. However, Deck and Nikiforakis (2010) find imperfect monitoring (when individuals can only observe the actions of their immediate neighbors in a circle network) cannot improve coordination efficiency. Although Devetag (2005)

number of contributors) can facilitate coordination. This is meaningful because we may not need perfect monitoring and associated punishments (e.g., Fehr & Gächter, 2000; Fudenburg & Pathak, 2011) (which are often costly and difficult) to improve coordination.

Remember that there are multiple dimensions of differences between experiments 1 and 2, as shown in Table 2.1. So is the different effect of gradualism really due to the different information feedback about exactly how many group members contribute? The analysis above for each specific group (see Figure 2.4) does show that the key is really regarding how the groups with three contributors perform differently in subsequent periods when they are told the exact number of contributors and when they are not. In addition, contribution rates in period 1 for each treatment between experiments 1 and 2 are very similar (see Figure 2.3), which shows that other settings of the experimental design are less likely to drive the difference of results in experiments 1 and 2 (except the different results of the “Semi-gradualism” treatment in period 7 between these two experiments, which is most likely due to whether the shock is anticipated, as shown in the aforementioned result 3). To further prove this, I ran several supplementary sessions the design of which is identical to that of experiment 2 except that subjects only know whether the project is successful but not exactly how many members contribute, as in experiment 1. The results (shown in Appendix A.5) are closer to those of experiment 1 than those of experiment 2, which confirms that the piece of information regarding the exact number of contributors is really the key.

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states all treatments in his study provide full information feedback, the information structure is actually still imperfect (as the richer information case in this chapter) since only the distributions of group members’ actions, but not individual choices, are revealed. He finds that such a richer information structure cannot help coordination. Dorsey (1992) and Kurzban et al. (2008) find a positive effect of real time perfect monitoring on a continuous public goods game and a trust game, respectively.

Note that in the first six periods, the stake of the “Gradualism” treatment is always increasing but that of the other treatments is constant. This may explain why in experiment 2 fewer “Gradualism” groups with three contributors become successful than “Big Bang” and “Semi-gradualism” groups do. For a group with three out of four members contributing, if the next stake is the same, they know they are close to success in the next period; but if the next stake increases (it is a small increase in the absolute sense but not in the relative sense), they may feel that they are even farther from success: if one group member does not contribute at a low stake, even if you allow a possibility that she has made a mistake (thus her real intention is to contribute that stake), you may hardly believe that there is a large chance she would contribute at a higher stake. If this is indeed the reason, a minor modification of the “Gradualism” treatment may make it work better: introducing repetitions for each lower stake. For example, the stakes over periods before the final high stake can be 2, 2, 4, 4, 6, 6, 8, 8, 10, 10, 12, 12 (each lower-stake project is replicated; for a more robust path, each can be replicated twice; another possible way, which might be less robust, is to have a smaller step). Under this new path, those “Gradualism” groups with three contributors may be more likely to become successful in the next period before it reaches the high stake. Then we may find a more obvious advantage of gradualism with a richer information structure than what is found in experiment 2, because gradualism guarantees a higher proportion of groups with four or three contributors (i.e., successful or almost successful) at the beginning: Table 2.4 (along with Figure 2.4) shows that in experiment 2, the number of “Big Bang” groups with four (or three) contributors in period 1 is six (or six) out of 18 groups, while those of “Semi-gradualism” and “Gradualism” groups are 10 (or seven) and 11 (or seven); similarly, in experiment 1, the numbers are three (or seven) for

“Big Bang” groups, 13 (or five) for “Semi-gradualism” groups, and 13 (or five) for “Gradualism” groups.

Table 2.4: Summary of Treatments in Experiments 1 and 2 (The Two Weakest-link Experiments)

Treatment	Experiment 1				Experiment 2		
	Big Bang	Semi-gradualism	Gradualism	High Show-up Fee	Big Bang	Semi-gradualism	Gradualism
Show-up Fee ( <i>yuan</i> )	10	10	10	12	10	10	10
Exchange Rate (points/ <i>yuan</i> )	40	40	40	40	30	30	30
Endowment per period (point)	20	20	20	20	20	20	20
Stake in period 1 (point)	14	2	2	14	14	2	2
Stake in period 6 (point)	14	2	12	14	14	2	12
Stake in periods 7-12 (point)	14	14	14	14	14	14	14
No. of groups	18	18	18	10	18	18	18
No. of subjects	72	72	72	40	72	72	72
No. of groups successful in period 1	3	13	13	3	6	10	11
No. of groups almost successful in period 1 (with three contributors)	7	5	5	3	6	7	7
No. of groups successful in period 6	3	13	12	3	10	14	13
No. of groups successful in period 7	3	6	12	3	10	14	13
No. of groups successful in period 12	3	6	11	3	11	9	11

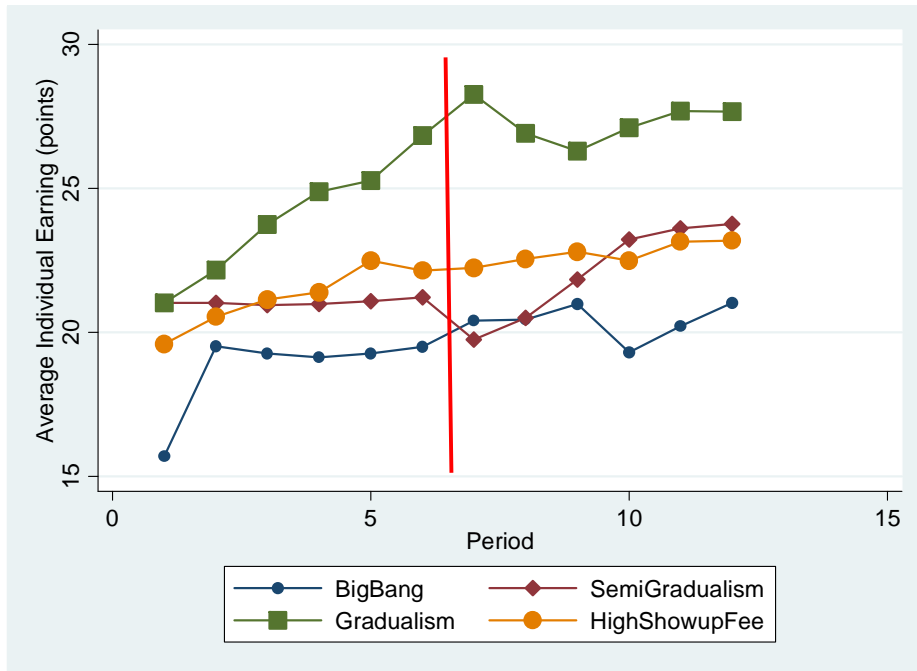
Note: Some numbers regarding experiment 1 are adopted from Chapter 1.

**Result 5: Conditional on successful coordination at  $t$ , most groups succeed with the same stake at  $t+1$ .**

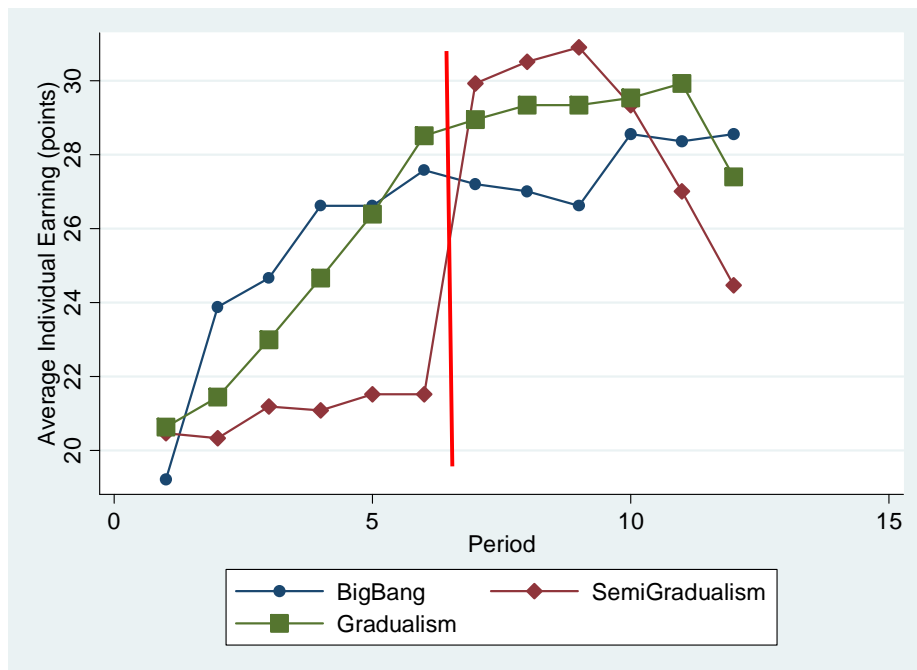
We can see this from Table 2.4. In experiment 1 (or 2), there are only seven (or 10) exceptions.

Figure 2.5 shows the average individual earnings, which helps answer whether gradualism works better from the perspective of social welfare. In experiment 1, clearly the “Gradualism” treatment has the highest average earning; in experiment 2, the difference between the “Big Bang” and “Gradualism” treatments is insignificant, while the average earning for the “Semi-gradualism” treatment is lower in the first six periods because of a lower earning potential (the stakes are low) but catches up with the other two treatments in the six high-stake periods.

Tables 2.5 and 2.6 report formal regressions for experiments 1 and 2, respectively. It clearly shows that gradualism works best in experiment 1, while its advantage is less obvious in experiment 2. I also employ probit and logit specifications (unreported) when the dependent variable is whether an individual contributes or whether a group succeeds in coordination, and the results are very similar. When the dependent variables are contributions and earnings, which are at the individual level, additional regressions (unreported) with survey controls show very similar results.



A. Experiment 1 (Limited-information Weakest-link; adopted from Chapter 1)



B. Experiment 2 (Richer-information Weakest-link)

Figure 2.5: Individual Earnings by Treatment and Period in Weakest Link Experiments



Table 2.5: Contribution, Earning and Success in Periods 7-12 by Treatments of Experiment 1  
(Limited-information Weakest-link Experiment)

	Individual Contribution			Individual Earning			Success (Group-level)		
	Period 7	Period 12	(1)	Period 7	Period 12	(2)	Period 7	Period 12	Period 7-12
Big Bang	-0.431*** (0.126)	-0.403*** (0.136)	(1)	-7.861*** (2.472)	-6.639*** (2.401)	(2)	-0.500*** (0.171)	-0.444*** (0.170)	-0.454*** (0.144)
Semi-gradualism	-0.056 (0.113)	-0.278* (0.152)	(3)	-8.528*** (3.076)	-3.889 (2.587)	(4)	-0.333* (0.185)	-0.278 (0.182)	-0.287* (0.157)
High Show-up Fee	-0.286* (0.158)	-0.292 (0.176)	(5)	-6.028* (3.203)	-4.467 (3.049)	(6)	-0.367* (0.199)	-0.311 (0.201)	-0.320* (0.186)
Constant	0.736*** (0.092)	0.667*** (0.107)	(7)	28.278*** (1.909)	27.667*** (1.925)	(8)	0.667*** (0.117)	0.611*** (0.121)	0.620*** (0.114)
Observations	256	256		256	256		64	64	384
R-squared	0.13	0.10		0.13	0.10		0.16	0.13	0.13

Note: This table is adopted from Chapter 1. The default treatment is “Gradualism,” so the constant shows the means for the “Gradualism” treatment, and the coefficients of “Big Bang,” “Semi-gradualism” and “High Show-up Fee” show the differences between those treatments and the “Gradualism” treatment. Robust Robust standard errors in parentheses. \* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%. Individual contribution and group-level success are binary variables. Standard errors are clustered at session level for (5) and (6), and clustered at group level for all other regressions. The observations are at individual level for regression (1)-(4), and at group level for regression (5)-(7). There are 256 subjects in 64 groups of 18 sessions. When we add survey controls for regression (1)-(4) where the observations are at individual level, the results (not reported) remain similar.

Table 2.6: Contribution, Earning and Success in Periods 7-12 by Treatments of Experiment 2  
(Richer-information Weakest-link Experiment)

	Individual Contribution			Individual Earning			Success (Group-level)		
	Period 7	Period 12	Period 7-12	Period 7	Period 12	Period 7-12	Period 7-12	Period 12	Period 7-12
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Big Bang	-0.208 <sup>a</sup> (0.139)	-0.083 (0.154)	-0.144 <sup>b</sup> (0.141)	-1.750 (2.742)	1.167 (2.632)	-1.361 <sup>d</sup> (2.397)	-0.167 (0.162)	-0.000 (0.167)	-0.120 <sup>e</sup> (0.155)
Semi-gradualism	0.042 (0.109)	-0.014 (0.137)	0.009 (0.124)	0.972 (2.725)	-2.917 <sup>c</sup> (3.164)	-0.389 (2.259)	0.056 (0.148)	-0.111 (0.169)	-0.009 (0.141)
Constant	0.806*** (0.084)	0.694*** (0.101)	0.759*** (0.090)	28.940*** (2.015)	27.390*** (2.066)	29.070*** (1.698)	0.722*** (0.109)	0.611*** (0.118)	0.704*** (0.105)
Observations	216	216	1,296	216	216	1,296	54	54	324
R-squared	0.064	0.006	0.024	0.018	0.035	0.005	0.041	0.011	0.013

Note: The default treatment is “Gradualism,” so the constant shows the means for the “Gradualism” treatment, and the coefficients of “Big Bang” and “Semi-gradualism” show the differences between those two treatments and the “Gradualism” treatment. Robust standard errors in parentheses. \* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%. Individual contribution and group-level success are binary variables. Standard errors are clustered at group level except for regressions 7-8. The observations are at individual level for regression (1)-(6), and at group level for regression (7)-(9). There are 216 subjects in 54 groups of 9 sessions. When we add survey controls for regression (1)-(6) where the observations are at individual level, the results (not reported) remain similar.

- a. significant at 1% if the standard error is not clustered.
- b. significant at 1% if the standard error is not clustered.
- c. significant at 10% if the standard error is not clustered.
- d. significant at 5% if the standard error is not clustered.
- e. significant at 10% if the standard error is not clustered.

## **2.5 Further Theoretical Discussions on Weakest-link Games**

### **2.5.1 The Myopic Assumption**

In the simple model, I adopt the myopic assumption. In reality, some players may be forward looking. For example, they may employ the following strategy: contributing at the beginning, thus inducing others to contribute subsequently. However, the myopic assumption does not affect the explanatory power of this simple model. The experimental results show that not all players (especially those with a high starting stake) use this strategy; so even such a strategy exists, it is more likely to be used when the risk is lower, which is consistent with the belief-based learning model. Moreover, even the number of periods and the stake path are anticipated in experiment 2 but not in experiment 1 (which suggests that this strategy should be more likely to happen in experiment 2), I find no obvious differences regarding the contribution rates in period 1 between experiments 1 and 2.

### **2.5.2 Evidence of Belief-based Learning in the Literature**

Although I do not formally test whether the subjects play according to their beliefs in this study, some recent papers confirm that the majority of, although not all, subjects behave consistently with their beliefs (e.g., Nyarko & Schotter, 2002; Costa-Gomes & Weizsäcker, 2008; Rey-Biel, 2009).<sup>43</sup> Direct belief elicitation methods, especially in an incentive-compatible way, become increasingly popular in experimental economics (e.g., Offerman et al., 1996, 2001; Nyarko & Schotter, 2002; Costa-Gomes & Weizsäcker, 2008; Rey-Biel, 2009; Hyndman et al., 2009; Tingley & Wang, 2010). I finally gave up doing this because

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<sup>43</sup> Fischbacher and Gächter (2010) study the dynamics of free riding in standard public goods experiments, and find that belief updating and social preferences are both important in explaining the results.

the main purpose of this study is to cleanly test whether gradualism works, and I had a concern that asking belief questions in each period would have undesirable effects on subjects' playing which may contaminate the results. The literature supports this concern: several other studies (Costa-Gomes & Weizsäcker, 2008; and especially, Rutström & Wilcox, 2004, 2009) do show that eliciting players' beliefs induce more sophistication and higher-order rationalities, thus affecting their actions.

The belief-based learning framework can also explain the main results of other minimum-effort coordination games in the literature. For example, Van Huyck et al. (1990) find that most subjects play the minimum reported in the earlier period, which suggests that they do update their beliefs about the minimum effort of group members according to the outcome of the previous period, and choose their best responses based on their beliefs. Moreover, by introducing a noise ("mistake") in playing, the framework can also explain a minor "overshooting" phenomenon (see pp 241, Section V of their paper) that a few subjects play below the minimum of the preceding period and choose the securest action: if the opponents may make mistakes with some probability, the real intention of the least-cooperative opponent may be to play below the observed minimum of the preceding period; even if his real intention is to play the observed minimum, in the next period he may make a mistake and choose a lower effort. Of course, this might not be the only way to explain the "overshooting" phenomenon.

This framework is also consistent with the main results of Weber (2006), who studies gradualism in organizational growth: starting with a large group size makes one believe that there is a low probability that all partners will choose a large effort, so it is less likely to reach a high equilibrium. On the contrary, starting with a small group size will give one a

high belief that *all* partners will choose a large effort.<sup>44</sup> And if a new group member knows the previous successful coordination history of the group, she will have a high belief that all partners will choose a large effort (and other partners, knowing that the entrant knows the history, will also have a high belief that the entrant will choose a large effort), so they can maintain a successful coordination when the group enlarges. However, if the entrant cannot observe the history, a high trust among the entrant and incumbents cannot be easily established, leading to a coordination failure when the group expands.

### **2.5.3 Other Explanations**

#### **2.5.3.1 Reinforcement Learning**

There are two main types of learning models: belief-based learning and reinforcement learning models, which are both incorporated in a general model of experience-weighted attraction (EWA) learning model (Camerer & Ho, 1999).<sup>45</sup> Unlike a belief-based learning model adopted in this chapter, reinforcement learning models assume that players do not have beliefs about what other players will do, and the propensity to choose a strategy depends on its past payoffs rather than the history of play that created those payoffs.

Previous studies (see the summary by Camerer, 2003) show that reinforcement learning generally fits experimental results worse than belief learning in coordination games. Similarly, a standard naïve reinforcement learning model does not apply well for the results

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<sup>44</sup> The larger the group size, the smaller the probability one perceives that *all* partners' efforts exceed a certain amount.

<sup>45</sup> There are other models such as the quantile response equilibrium (QRE) model (McKelvey & Palfrey, 1995). As suggested by Brandts and Cooper (2006b), given the strong dynamics and history dependence in the experimental data of this study, static models such as QRE are not good candidates. Thus I focus on learning models in which some players have bounded rationality and learn how to respond from their experiences.

of experiment 1. It predicts that subjects who benefit from cooperating by period 6 should adopt the passive strategy of continuous cooperation from period 7; but this contrasts the findings regarding the “Semi-gradualism” treatment.

### 2.5.3.2 An Adaptive Dynamics Model

Crawford (1995) and Weber (2005, 2006) adopt an adaptive dynamics model, in which every subject employs a linear latent strategy weighting own action and the minimum action of group members in the previous period.<sup>46</sup> Their model does not apply for this study for two reasons. First, in their experiments, there are seven action choices in each period, making a linear continuous latent strategy more relevant; in this study there are only two choices in each period, which is “more discrete.” Second, in their experiments, the game is repeated identically for all periods; in this study the stake may change over periods, so a linear weighting of previous actions cannot well predict or guide actions in the current period.

### 2.5.3.3 Conditional Cooperation

Conditional cooperation assumes that players are willing to cooperate if others cooperate as well (e.g., Fischbacher et al., 2001). Based on the same reason in Section 2.5.3.1, a simple story of conditional cooperation does not work, either. A weaker version of conditional cooperation may fit the results better: if others cooperate in the previous period, then own cooperation in the current period leads to a higher non-monetary psychological

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<sup>46</sup> Specifically, in their models, player  $i$ ’s latent strategy in period  $t$  is given by the following formula:  $a_{i,t} = (1 - \beta)x_{i,t-1} + \beta y_{t-1} + \varepsilon_{i,t}$ , where  $x_{i,t-1}$  is  $i$ ’s discrete action in period  $t-1$ ,  $y_{t-1}$  is the minimum action of group members in period  $t-1$ , and  $0 < \beta < 1$ . The  $\varepsilon_{i,t}$  are distributed normally, with mean zero and variance  $\sigma_i^2$ . Player  $i$ ’s discrete action  $x_{i,t}$  in period  $t$  is determined by the latent variable  $a_{i,t}$  (e.g., by rounding  $a_{i,t}$  to the nearest discrete action).

gain than non-cooperation, but it does not necessarily mean that one would keep cooperating. Whether one will choose to cooperate depends on the relative importance of conditional cooperation and self-interest in her preference. If the stake increases too quickly, then one will choose not to cooperate if she has a low belief that others will still cooperate at a much higher stake and if self-regarding dominates conditional cooperation in her preference structure.

Conditional cooperation sometimes means that players are willing to cooperate if she *believes* that others will cooperate as well. By this definition, I cannot really distinguish conditional cooperation and self-interest in these two experiments, because under the weakest-link payoff structure, the best response for a self-interested person is to cooperate if she believes that all others will cooperate. However, even such conditional cooperation exist beyond a pure self-interest preference, it is compatible with the belief-based learning process. In Section 7 I present a third experiment with the chance for free riding and find that coordination often collapse after success, which shows that conditional cooperation may not apply very well, so what drives subjects' behaviors in these experiments is more likely to be belief updating.

#### **2.5.3.4 Inertia**

Psychological inertia<sup>47</sup> refers to indisposition to change. It is not successful at explaining why “Semi-gradualism” groups in experiment 1 cannot maintain successful coordination when the stake increases quickly.

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<sup>47</sup> Inertia has been studied in behavioral public finance issues such as individual saving behaviors (e.g., Madrian & Shea, 2001; Thaler & Benartzi, 2004).

### **2.5.3.5 Preferences for Consistency**

A similar psychological theory is preferences for consistency (Cialdini et al., 1995), which means that once people make a choice or take a stand, they encounter personal and interpersonal pressures to behave consistently with that commitment. Similar with inertia, it cannot well explain why “Semi-gradualism” groups in experiment 1 fail to maintain successful coordination.

### **2.5.3.6 Limited Attention and Bounded Awareness**

Since the stake step in the “Gradualism” treatment is small, is the gradualism effect (which is more obvious in experiment 1) due to limited attention (for a review, see DellaVigna, 2009), or bounded awareness (Gino & Bazerman, 2009)? The answer is no. First, subjects were doing the single coordination task in this study, so there was no other information or tasks which would distract their “limited attention.” Second, although the absolute increment of the stake in the gradualism treatment is small (two points per period), it is significantly larger than those in the experiments of Gino and Bazerman (2009) and Offerman and van der Veen (2010), which adopt much more gradual settings. Actually, in this study, the step of two is quite a large increase in the relative sense, ranging between 100% of the stake size in period 1 and 16.7% of the stake size in period 6.

## **2.6 When Free Riding Is Possible**

To examine whether gradualism (with a richer but imperfect information structure) helps build coordination (or “cooperation”) when free riding is possible, I conducted a third experiment. In experiment 3, in each period it requires at least three contributors in a four-



person group to make the project successful. It can be classified as a discrete public good game or a coordination game.<sup>48</sup> It was conducted in May 2011 at the same location as experiment 2. For other features, experiment 3 is identical to experiment 2; moreover, subjects in experiment 3 are comparable to those in experiment 2 (see Table 2.2). There are six sessions, each of which includes 24 subjects (two groups for each treatment). In total I have 12 groups for each treatment.

The payoff structure of experiment 3 is similar to that of experiments 1 and 2 except for the potential for free riding. If at least the required number (which is four in experiments 1 and 2, and three in experiment 3) of group members contribute, everyone in the group gets back twice the stake no matter whether she contributes; otherwise she loses her contribution. If more than the required number of group members contribute, then extra contributions will be equally divided among all group members.<sup>49</sup> The following formula (in the instruction, the payoff structure is presented in texts and a table, which is easier for subjects to understand; see Appendix A.1) describes individual earnings in each period of experiment 3:<sup>50</sup>

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<sup>48</sup> Palfrey and Rosenthal (1984) and Schram et al. (2008) are two discrete public goods studies which also adopt a binary choice set. It can be classified as an order-statistic game, one type of coordination games, since a player's payoff depends on her own choice and an order statistic of all group members' contributions (here the order statistic is the second-to-minimum contribution; in the weakest-link games, it is the minimum.)

<sup>49</sup> This rebate method is called "proportional rebate" in Marks and Croson (1998).

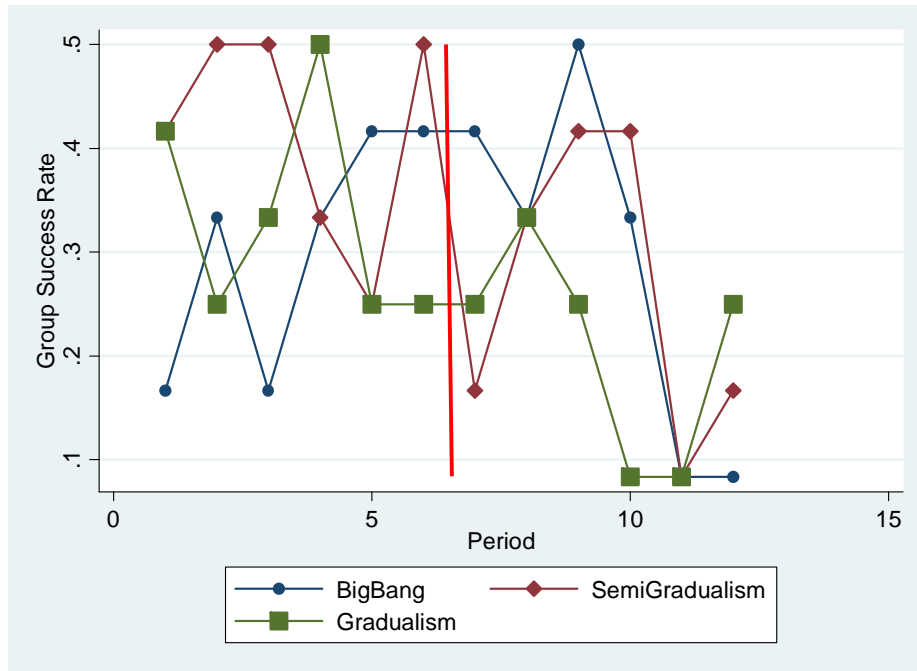
<sup>50</sup> As in experiment 2, the program randomly picks up one period's earning in the first stage (multiplied by the number of periods in the first stage) as the performance payment for that stage.

$$Earning_{i,t} = \begin{cases} 20 + 1.25Th_t, & \text{if } A_{i,t} = C \text{ and } A_{j,t} = C, \forall j \neq i \\ 20 + Th_t, & \text{if } A_{i,t} = C \text{ and } \sum_{j \neq i} 1(A_{j,t} = C) = 2 \\ 20 - Th_t, & \text{if } A_{i,t} = C \text{ and } \sum_{j \neq i} 1(A_{j,t} = C) < 2 \\ 20 + 2Th_t, & \text{if } A_{i,t} = D \text{ and } A_{j,t} = C, \forall j \neq i \\ 20, & \text{if } A_{i,t} = D \text{ and } \exists j \neq i, \text{ s.t. } A_{j,t} = D \end{cases}$$

where  $Earning_{i,t}$  is  $i$ 's earning in period  $t$ ,  $Th_t$  is the stake at  $t$ ,  $A_{i,t}$  and  $A_{j,t}$  are the actions of  $i$  and  $j$  at  $t$ , respectively ( $i$  and  $j$  are in the same group.)  $C$  represents “cooperate” (“give”), while  $D$  represents “deviate” (“not give”). The function  $1(A_{j,t} = C)$  has a value one if  $A_{j,t} = C$ , and zero otherwise.

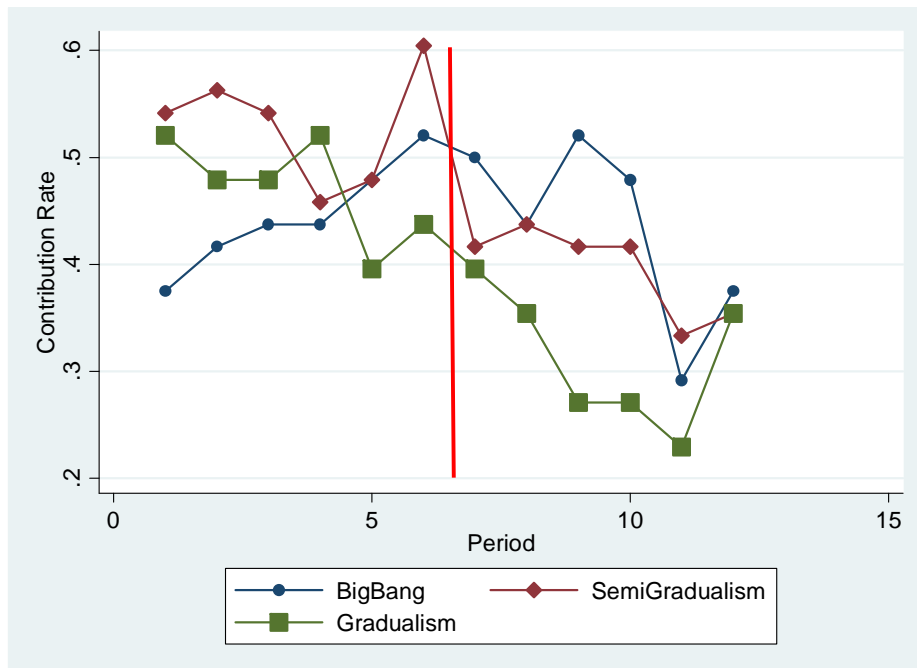
Figure 2.6 shows the success rate, contribution rate, and average individual earning for each treatment. Figure 2.7 shows the coordination result (the number of contributors) for each group by treatment. For each group, the horizontal axis indicates the period, and the vertical axis indicates the number of contributors. On the top of the graph for each group is the 2-digit or 3-digit code for that group, which is constructed in the following way: the lowest digit indicates the treatment type (1=“Big Bang,” 2=“Semi-gradualism,” 3=“Gradualism”); the highest one or two digits indicate the session number (1-6).

We can see lots of fluctuations in Figures 2.6 and 2.7: equilibrium is much less stable, and switching from one to another or to a non-equilibrium outcome is common. All results confirm the above concern and clearly show that all treatments do poorly at a high stake. Although gradualism has higher contribution and success rates at the beginning, coordination falls apart soon.



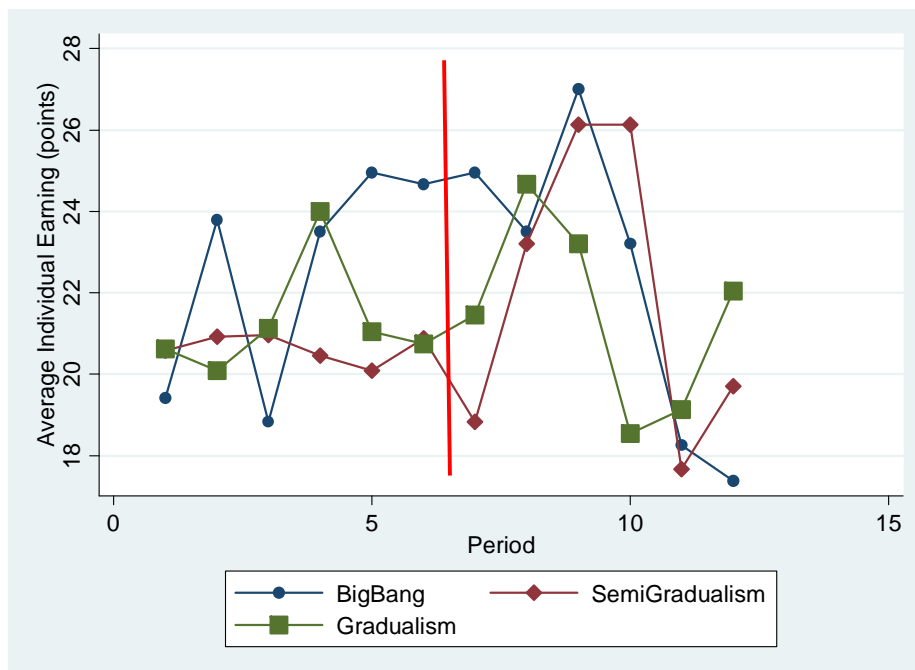
A: Success Rates

A group is successful in coordination if three or four members give the stake in that period.



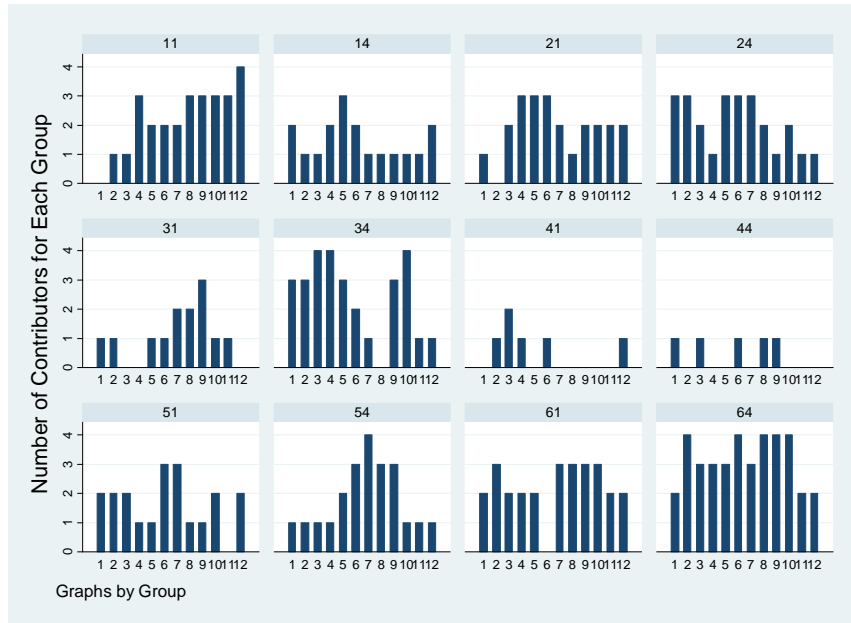
B: Contribution Rates

Figure 2.6: Performances of Treatments in Experiment 3 (with a Chance for Free Riding)

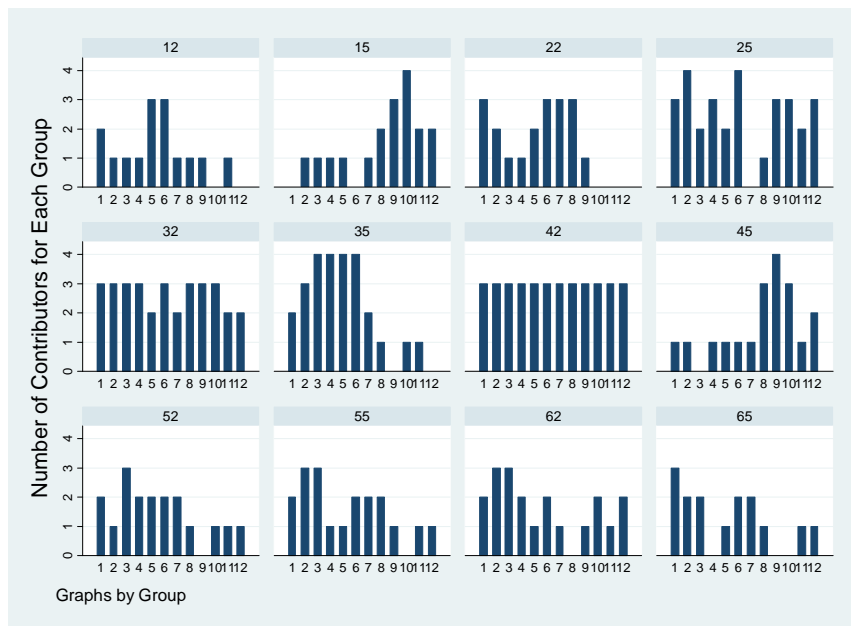


C: Individual Earnings

Figure 2.6 (Continued)

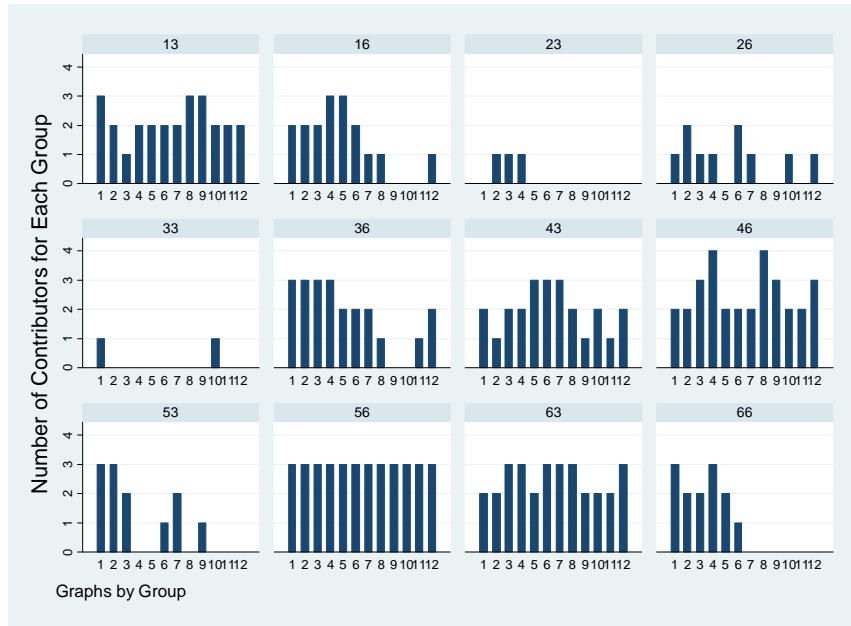


A: “Big Bang” Groups



B: “Semi-gradualism” Groups

Figure 2.7: Group Coordination Results for Each Treatment in Experiment 3 (With a Chance for Free Riding)



C: “Gradualism” Groups

Figure 2.7 (Continued)

Table 2.7 shows the performances of the treatments in periods 7-12 of experiments 2 and 3. In experiment 3 the gradualism groups have a lower contribution rate, and do not perform better regarding success rates and earnings: gradualism alone cannot build coordination when free riding is possible.

The lower panel of Table 2.7 compares results of experiments 2 and 3, and shows a surprising (but understandable) finding that coordination becomes more difficult rather than easier when we allow free riding. Actually, allowing free riding worsens the performances of all treatments. Not only the contribution rates, but also the success rates and individual earnings are much lower than the weakest-link case.<sup>51</sup> For example, the contribution rates in

<sup>51</sup> Schram et al. (2008) show that fixing the required number of contributors and given that a chance for free riding exists, the larger the group, the lower the contribution rate, but the higher the success rate. Experiments 2 and 3 differ from their study in both the experimental design and the result. First, I fix the group size but change the required number of contributors, and free riding is possible only in experiment 3. Second, this study shows that a lower required number of contributors decreases not

periods 7-12 of “Big Bang,” “Semi-gradualism” and “Gradualism” treatments in experiment 3 are 18.2, 37.3 and 44.7 percentage points lower than those in experiment 2, respectively; average individual earnings per period in periods 7-12 in experiment 3 are 5.3, 6.7 and 7.6 points lower (given an endowment of 20 points per period) for these treatments;<sup>52</sup> success rates are 29.2, 43.1 and 49.5 percentage points lower. All these differences are economically substantial and statistically significant, and larger for the gradualism treatment.

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only the contribution rate, but also the success rate. This suggests that the effect of the gap between the required number of contributors and the group size on the success rate may be nonlinear, and whether free riding is possible may be a turning point.

<sup>52</sup> The average payment (including the show-up fee) for experiments 3 was 24.9 *yuan*, one *yuan* (or 30 points) lower than that for experiment 2.

Table 2.7: Comparison of Treatment Performances in Experiments 2 and 3 (without vs. with a Chance for Free Riding)

	Individual Contribution Periods 7-12	Individual Earning Periods 7-12	Success (Group-level) Periods 7-12
	(1)	(2)	(3)
Mean Performances in Experiment 2			
a. Big Bang	0.616*** (0.023)	27.713*** (0.377)	0.583*** (0.048)
b. Semi-gradualism	0.769*** (0.020)	28.685*** (0.418)	0.694*** (0.045)
c. Gradualism	0.759*** (0.021)	29.074*** (0.393)	0.704*** (0.044)
Mean Performances in Experiment 3			
d. Big Bang	0.434*** (0.029)	22.382*** (0.672)	0.292*** (0.054)
e. Semi-gradualism	0.396*** (0.029)	21.944*** (0.655)	0.264*** (0.052)
f. Gradualism	0.313*** (0.027)	21.507*** (0.586)	0.208*** (0.048)
Total observations	2,160	2,160	540
Treatment Effects in Experiment 3			
Gradualism - Big Bang (f-d)	-0.122 (0.104)	-0.875 (2.370)	-0.083 (0.124)
Gradualism - Semi-gradualism (f-e)	-0.083 (0.098)	-0.438 (2.465)	-0.056 (0.128)
Semi-gradualism - Big Bang (e-d)	-0.038 (0.087)	-0.438 (2.543)	-0.028 (0.125)
The Effect of Allowing Free Riding (Compare Experiment 3 and Experiment 2)			
Big Bang - Big Bang (d-a)	-0.182 (0.128)	-5.331** (2.425)	-0.292* (0.143)
Semi-gradualism - Semi-gradualism (e-b)	-0.373*** (0.103)	-6.741*** (2.385)	-0.431*** (0.131)
Gradualism - Gradualism (f-c)	-0.447*** (0.121)	-7.567*** (2.349)	-0.495*** (0.138)

Note: Robust standard errors in parentheses, clustered at group level. \* significant at 10%, \*\* significant at 5%, \*\*\* significant at 1%.



The result of experiment 3 shows that gradualism *alone* without perfect monitoring cannot help when free riding is possible. Since the information structure in experiment 3 is still imperfect, we cannot rule out the possibility that gradualism works for the free riding case when there is perfect monitoring (i.e., subjects are provided the feedback about who have contributed) or when it is not conducted anonymously. It will be interesting to test whether gradualism becomes helpful when we combine it with other institutions (e.g., perfect monitoring, sanctions, social pressure, etc.). In addition, there might be other “gradualism” approaches which apply in other real world examples.

In experiment 3, coordination often collapses after success, which shows that conditional cooperation does not apply well, so what drives subjects’ behaviors in all three experiments is more likely to be belief updating. Gradualism works better in the weakest-link coordination games because belief updating across periods is more consistent, but not when free riding is possible because belief updating becomes much less consistent and much more difficult.

## **2.7 Conclusions**

The findings in this chapter show that gradualism -- defined as increasing the stake level required for coordination step-by-step -- can serve as a powerful mechanism for achieving socially optimal outcomes in weakest-link coordination especially when the information feedback is limited: gradualism significantly outperforms the *big-bang* approach, which shows that starting at a low level is important for later coordination at a substantial level. In addition, a gradual increase in stake is better than a sudden increase in one weakest-link game when the shock to the stake is unanticipated.

The role of gradualism is less obvious when the information feedback is richer. Detailed analyses of experimental data and theoretical discussions show that gradualism works through gathering the key information about how far a group is from success in a high-stake coordination project. A richer information structure also provides such information, so the advantage of gradualism is most obvious when feedback is limited. I also suggest that a more gradual stake path of the “Gradualism” treatment may work better: the bottom line is that by starting at a low stake gradualism guarantees a higher proportion of successful or almost-successful groups at the beginning, and a more gradual path not only maintains successful coordination, but also may switch a group from failure to success. This hypothesis can be tested in a future experiment.

Compared to the first experiment, the second experiment with a richer information structure shows that a further piece of (but still imperfect) information largely facilitates coordination (especially for the *big-bang* treatment), which has important implications for policy designs because perfect monitoring is often costly and difficult, as the literature on contract theory and organizational economics shows. Monitoring in the second experiment with a richer information structure is still imperfect in two senses. The first is that subjects do not know which members (identified only by codes) contribute (and which do not). The second is that the experiment is conducted anonymously, so subjects face no or little social pressure.

I propose a belief-based learning framework, which is consistent with results of the two weakest-link games in this chapter and other studies on coordination games such as Van Huyck et al. (1990) and Weber (2005, 2006). These findings are not well explained by naïve

reinforcement learning, conditional cooperation, inertia, preferences for consistency, and limited attention (or at least standard versions of these explanations).

I do not formally test belief-based learning in this chapter, which I show is the most likely mechanism of gradualism in weakest-link games. Although the belief elicitation method in the literature is imperfect, in the future I may use it to suggestively show whether belief updating is occurring and whether agents play according to their beliefs. I can also test this by exploring whether gradualism helps most for people who do not know their group members rather than for those who are friends of each other before the experiment, since the latter group already has a higher trust level even without gradualism.

Unfortunately, the optimistic roles of gradualism and a richer information structure in coordination are not found when free riding is possible. Although gradualism leads to a higher success rate at the beginning with a lower stake, this advantage cannot sustain. Actually all treatments perform worse than in the weakest-link case. I interpret this result in the way that gradualism *alone* does not help when free riding is possible. Future studies are desired to examine whether gradualism works under this case when we combine it with perfect monitoring, social pressure, sanctions or other institutions.

This chapter builds on Chapter 1 and uses various information and payoff structures to explore when gradualism works and why, and suggests how to make it work better. It shows the positive roles of richer information feedback and the weakest-link payoff structure in coordination. It also suggests that the popular hypothesis of gradualism cannot be taken for granted: the effect of gradualism varies across different conditions.

### **3. DOES GRADUALISM BUILD TRUST? EVIDENCE FROM A MULTI-ROUND EXPERIMENT**

#### **3.1. Introduction**

Trust, defined by Mayer et al. (1995) as “the willingness of a party to be vulnerable to the actions of another party based on the expectation that the other will perform a particular action important to the trustor, irrespective of the ability to monitor or control that other party,” is viewed as a lubricant for social interactions and market efficiency (e.g., Arrow, 1974; Putnam, 1993; Fukuyama, 1995; La Porta et al., 1997; Knack & Keefer, 1997; Guiso et al., 2007). However, untrustworthiness of some trustees will harm the trusting behavior and discourage trust. For one-shot interactions, self-interested rational agents expect that strangers will be untrustworthy and choose not to trust. Thus it may not be too surprising that low levels of trust are generally found in the experimental literature (see, e.g., Camerer, 2003; Bohnet & Croson, 2004; Kramer & Cook, 2004; Bohnet, 2008).

This chapter experimentally studies a potential mechanism to promote trust, as well as the evolution and interaction of trust and trustworthiness over time. Specifically, it focuses on the hypothesis of gradualism (or incrementalism) which suggests that a high level of trust should be built up slowly rather than immediately. This issue may be theoretically complex. On the one hand, standard game theory suggests that in the stage game, the subgame perfect equilibrium is for the trustor not to trust and for the trustee to be untrustworthy if the trustor trusts. Playing these actions in each round is the unique subgame perfect equilibrium for finitely interactions, no matter how stakes change over time. On the other hand, when

players have other-regarding preferences like reciprocity, they may behave inconsistently with the standard theoretical prediction. For example, one can imagine that the trustee belongs to either a “reciprocal type” or a “selfish type,” which is unknown to the trustor. In a repeated setting, the reciprocal trustee will always reciprocate the trustor, while the selfish trustee, with a high probability (as long as the time horizon is long enough), will try to hide his type by reciprocating the trustor until a certain round so that the trustor does not stop trusting. A low stake at the beginning makes it cheap for the trustee to give up immediate gains from betrayal and for the trustor to take a risk in investing; once successful cooperation is established, it may maintain with slightly larger stakes. But as the game proceeds towards the end round, the probability that the selfish trustee betrays approaches one.

Previous research either supports (e.g., Rapoport et al., 2003; Roberts & Renwick, 2003; Cocharde et al., 2004; Bohnet & Meier, 2005; Johansson-Stenman et al., 2005; Kurzban et al., 2008) or doubts (Pillutla et al., 2003; Pitchford & Snyder, 2004; Ho & Weigelt, 2005) this gradualism hypothesis (see Section 3.2 for details), but none of them provides a clear test how exogenous early stake paths affect trust and trustworthiness in final high-stake interactions. Thus this chapter employs an empirical (experimental) method and tries to explore how trust and trustworthiness evolve under various stake paths.

In the experimental literature, trust and trustworthiness are measured by investment or trust games introduced by Berg et al. (1995) (for reviews, see Bohnet, 2008; Cardenas & Carpenter 2008). The trustor is endowed with some monetary units and asked to pass an amount (investment) up to the endowment; the amount sent is multiplied by some factor  $k > 1$  (say, tripled). Then the trustee (either endowed or not) is asked to return an amount up to the

total output. Trust is measured by the ratio of investment to the trustor's endowment, while trustworthiness is measured by the ratio of returned amount to the investment (or output). Although this measure may not measure pure trust, it is a standard experimental measure and captures substantial aspects of trust (Ben-Ner & Halldorsson, 2010; Brühlhart & Usunier, 2012).<sup>53</sup>

This chapter adopts a binary variant<sup>54</sup> of the trust game to test the gradualism hypothesis. In each round of the experiments, subjects (both trustors and trustees) are endowed with some money. First, the trustor is asked to invest a certain amount (referred to stake) on the randomly matched trustee. She<sup>55</sup> can only choose not to invest or to invest exactly the stake. If the trustor does not invest, both the trustor and the trustee end the round with their endowments. Otherwise the investment triples and the trustee decides how much to return to the trustor. His decision is also binary: to return either an unfair amount (half of the stake) or a fair amount (twice the stake). If he returns an unfair amount, the trustor loses half the stake from the endowment, while the trustee earns twice and half of the stake beyond the endowment; if he returns the fair amount, each of them earns the stake beyond the endowment, which leads to an equal outcome. No sending and returning an unfair

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<sup>53</sup> The trust game may measure neither the trustor's pure expectation (i.e., trust attitude) about the trustee's trustworthiness nor the trustee's reciprocity. The trust measure may also involve unconditional kindness (Cox, 2004; Ashraf et al., 2006), risk aversion (Schechter, 2007), and betray aversion (Bohnet & Zeckhauser, 2004; Bohnet et al., 2008). The trustworthiness measure may involve altruism (Cox, 2004) and inequality aversion (Ciriolo, 2007). In repeated games, they may also involve strategic considerations rather than pure trust and trustworthiness. In this chapter I follow the tradition and use the term "trust" and "trustworthiness," but refer to them as cooperative behaviors of trustors and trustees in the investment setting rather than their preferences or attitudes.

<sup>54</sup> Some studies (e.g., Bohnet & Zeckhauser, 2004; Engle-warnick & Slonim, 2004; Bohnet et al., 2008) also adopt a binary structure.

<sup>55</sup> For ease of understanding, I refer to the trustor as "she" and the trustee as "he."

amount is the subgame perfect equilibrium of the trust game, given that the players are mere money-maximizers.

There are 12 rounds in this experiment. As in Chapters 1 and 2, the path of the stake, which may or may not change over rounds, is determined by the exogenously assigned treatment. I introduce three main treatments of stake patterns, which differ in the first six rounds but have an identical stake for the final six rounds. The first treatment has a constant high stake for all 12 rounds; I call this “Big Bang.” The second treatment has a constant low stake for the first six rounds and jumps to the high stake for the final six rounds; I call this “Semi-gradualism.” Finally, in the “Gradualism” treatment, the stake gradually increases in each of the first six rounds until it reaches the high stake in round 7. Note that the “Semi-gradualism” treatment falls between the “Big Bang” and “Gradualism” treatments: its stake starts at low but has a sudden increase. Exploiting this design, I test the effect of gradualism on trust building at a high stake.

I find that at the beginning the trust rate is the same for all treatments, but more trustees are untrustworthy when the starting stake is high; as a result, the “Big Bang” treatment has a lower level of subsequent trust. Interestingly, in the second half of the experiment (rounds 7-12) with the high stake, trustworthiness is similar across treatments. There is no performance difference between a slow increase in stake (the “Gradualism” treatment) and a sudden increase (the “Semi-gradualism” treatment). Finally, the gradualism approach cannot avoid the “end-of-game” effect: trustworthiness and trust for all treatments collapse in the last round.

The rest of the paper is organized as follows. In Section 3.2, I discuss the literature on dynamic trust games. In Section 3.3, I detail the experimental design. In Section 3.4, I present the major results. Section 3.5 concludes.

### **3.2. Literature on Dynamics in Trust Games**

This section discusses more about the literature on the effects of stake size and stake path in trust games. To be focused, I largely ignore the literature on dynamics of other games, which can be found in Chapters 1 and 2.

The gradualism hypothesis implies that a lower stake should lead to higher trust and trustworthiness, which are confirmed in some studies and but not in others. Johansson-Stenman et al. (2005) study the comparative statistics of stakes in trust games and find that the trust level decreases with the stake. Similarly, in a centipede game (two subjects play the roles of “trustor” and “trustee” in turn), Rapoport et al. (2003) report that when the number of players increases from two to three and stakes are sufficiently high, defection occurs very early in the interaction. They also find that players are more reluctant to trust in later stages than in earlier ones and are more trustworthy if they are certain of the trustor’s intention. On the contrary, Ho and Weigelt (2005) study a multistage trust game which is close to a centipede game, and find that subjects are more trusting and trustworthy when the stake size increases tenfold.

Some evidences suggest that players endogenously choose to gradually increase their trust levels. Roberts and Renwick (2003) present a series of games resembling the prisoner’s dilemma in which investment can be varied across rounds. They find that investment increases over the course of successive rounds, suggesting that players use a strategy of



gradually increasing their trust if previous investment in their partner is reciprocated. Cocharde et al. (2004) find that trustors (trustees) send (return) a larger ratio than in the one-shot game; in addition, the amount sent and the proportion returned increase over time. Bohnet and Meier (2005) compares a distrust game with a trust game, and find that the same level of trust leads to lower trustworthiness when the default is full trust than when the default is no trust, which suggests that building trust incrementally may be a better way for the trustor. Kurzban et al. (2008) show that subjects prefer starting with smaller levels of investment and increasing it, rather than the other way around.

In contrast, other studies suggest that the gradual way of trust building may not necessarily work. In a one-shot trust game, Pillutla et al. (2003) demonstrate that showing a large amount of trust in a one-shot interaction leads to more reciprocation than showing intermediate amounts of trust. Specifically, they find that trustors only benefit (i.e., trustees return more than trustors send), on average, when they send all or almost all of their endowments. Results suggest that trustees view sending less than (almost) everything as a lack of trust and thus not feel obligated to reciprocate enough to reward the trustor. This contrasts with incremental models of the trust process, which suggest that trustors should take small initial risks and build trust gradually. Pitchford and Snyder (2004) develop a theoretical model where the sequence of gradually smaller investments solves the holdup problem when the buyer's ability to hold up a seller's investment is severe. However, this prediction contradicts the experimental results in Kurzban et al. (2008) aforementioned.

Building on the literature, this chapter experimentally explores whether an exogenous gradual stake path helps build up trust and trustworthiness. The exogenous way helps

answer whether a gradual path better builds high-level trust, rather than whether it is chosen by players.

### **3.3. Experimental Designs**

#### **3.3.1. Sample and Payoff Structure**

The design of the trust experiment is similar with the coordination experiments in Chapters 1 and 2. It was conducted at the Finance and Economics Experimental Lab at Xiamen University in Xiamen, China in July 2011 with 120 subjects recruited via the Online Recruitment System for Economic Experiments (ORSEE) at the university. Table 3.1 shows the demographic characteristics of subjects. The subjects are generally young with an average age around 21, since 95% of them are college or graduate students. Exactly half of them are male. 8% are (or were) majored in economics, 7% in other social sciences, 24% in business, 13% in humanities, 25% in science, 17% in engineering, and 3% in medical/health. The average individual annual income in the year of 2010 falls between 5,000 yuan and 10,000 yuan. Thus subjects' characteristics are similar to those in Chapters 1 and 2, except that the distribution of concentration is not identical.

Table 3.1: Demographic Characteristics of Subjects in the Trust Game

Variable	Mean and standard deviation	Observations
Age	21.32 (2.52)	120
Male	0.50 (0.50)	120
Income	1.20 (1.15)	120
Family Income	5.52 (2.90)	120
Family Economic Status	2.44 (0.89)	120
Risk Aversion Index	4.85 (1.79)	120
Han nationality	0.93 (0.25)	120
Student	0.95 (0.22)	120
<b>Concentration:</b>		120
Economics	0.08 (0.28)	120
Other Social Sciences	0.07 (0.25)	120
Business	0.24 (0.43)	120
Humanity	0.13 (0.34)	120
Science	0.25 (0.43)	120
Engineering	0.17 (0.37)	120
Medical/Health	0.03 (0.18)	120

Note: Income is a scale variable from 0 to 13, with a higher value indicating a higher income (0: no income; 1: annual income<5000 *yuan*; 13: annual income>160,000 *yuan*). Family income is a scale variable from 1 to 12, with a higher value indicating a higher income (1: annual income<5000 *yuan*; 12: annual income>200,000 *yuan*). Family economic status is coded in the following way: 1 (lower), 2 (lower middle), 3 (middle), 4 (upper middle), 5 (upper). Risk aversion index is a scale from 0 to 10, with a higher value approximately indicating higher risk aversion, and is measured as the number of lottery A chosen by the subject in the questionnaire (see Appendix A.2).

As in coordination games in Chapters 1 and 2, all laboratory sessions were computerized using the z-Tree experiment software package (Fischbacher, 2007). Both the instructions and the information shown on the computer screen were in Chinese. The instruction handouts (see Appendix A.6) were read aloud for everyone in the same session to hear. Each session lasted between 30 minutes and one hour (including the time for signing up, reading instructions, a quiz to guarantee the accurate understanding of the experimental rules, and final payments).

The experiment included 7 sessions (each had 6, 12, 18 or 24 subjects). In each session, I randomly assigned subjects to groups of two. Each session included 12 rounds. Group compositions were fixed during the experiment; this rule was made common knowledge. In each group, the two subjects play two different roles “Person A” and “Person B,” respectively. I used the natural terms “Person A” and “Person B” rather than “Trustor” and “Trustee” in the instruction to avoid any experimenter demand effect. The role was determined randomly by the computer at the beginning of the experiment. Each subject always played the assigned single role across the whole experiment. However, each group only knew their own stake path, but not those of other groups.

In each round, I endowed each subject with 14 *yuan*.<sup>56</sup> Then “Person A” was asked to pass  $X$  *yuan* to “Person B.” This number ( $X$ ) might (or might not) vary over rounds and can be any even number between 0 and 14 (including 0 and 14), and it was known by both “Person A” and “Person B.” The path of  $X$  in all rounds depended on the treatment, and was told to subjects when the experiment started.

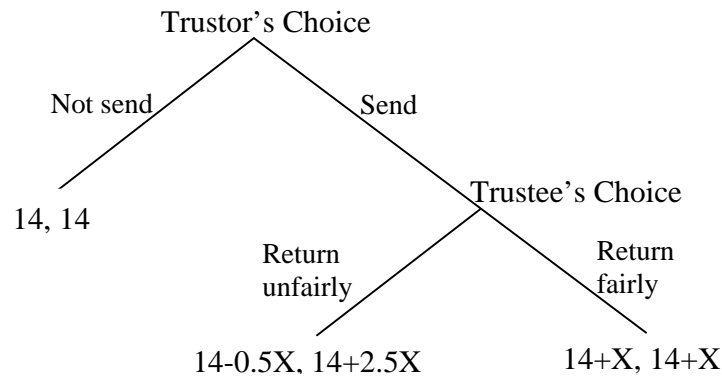
“Person A” first decided whether to pass  $X$  *yuan* or to pass nothing to “Person B.” That is to say, the amount of money that “Person A” passed should be either 0 or  $X$  *yuan*, not any other amount. If “Person A” chose NOT to pass  $X$  *yuan*, then each of “Person A” and “Person B” ended the round with 14 *yuan*; otherwise the  $X$  *yuan* sent out would be tripled to be  $3X$ , and “Person B” then decided how much to return to “Person A.” “Person B” also had two choices: either to return  $2X$  *yuan*, or to return  $0.5X$  *yuan*. If “Person B” chose to return  $2X$  *yuan*, “Person A” ended the round with  $14 - X + 2X = 14 + X$  *yuan*, and “Person B” ended the

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<sup>56</sup> In many trust games only trustors are endowed, which may underestimate trustees’ trustworthiness if they are inequality averse, and trustors’ trust if they expect the trustees have such an inequality aversion preference (Ciriolo, 2007). I adopt the design of equal endowment between trustors and trustees to avoid this confounding factor.

round with  $14+3X-2X=14+X$  *yuan*; so they earned an equal amount. If “Person B” chose to return  $0.5X$ , “Person A” ended the round with  $14-X+0.5X=14-0.5X$  *yuan*, and “Person B” ended the round with  $14+3X-0.5X=14+2.5X$  *yuan*, respectively. Thus passing the stake is a trusting behavior, and returning twice the stake is a trustworthy one, while returning only half the stake harms the trustor and is considered as untrustworthy.

The above rule of each round was shown in Figure 3.1.



The first (second) number in the terminal nodes indicates the earning of the trustor (trustee) in that round.

Figure 3.1: The Decision Tree in Each Round

In each round, the actions of “Person A” and “Person B” were known to each other immediately after the action. However, communication across subjects was not allowed.

The final payment was the performance payment plus a show-up fee of 10 *yuan*. At the end of the experiment, the computer randomly picked up one round from the 12 rounds; the performance payment was the earning in the chosen round.

The average payment (including the show-up fee) was 29.2 *yuan* and 33.4 *yuan*<sup>57</sup> for “Person A” (trustors) and “Person B” (trustees), respectively. The experimental design contributes to the advantageous position of trustees: no matter what choices are made by

<sup>57</sup> The exchange rate when the experiments were conducted was about 1USD=6.5 *yuan*.

trustors and trustees, trustees never earn less than his partners (trustors), as shown in Figure 3.1. These earnings could afford ordinary meals at student canteens for one to three days, and were sufficiently large to generate a good supply of subjects. Regarding the purchasing power, it is comparable to experimental payments (for one-hour experiments) in other countries.

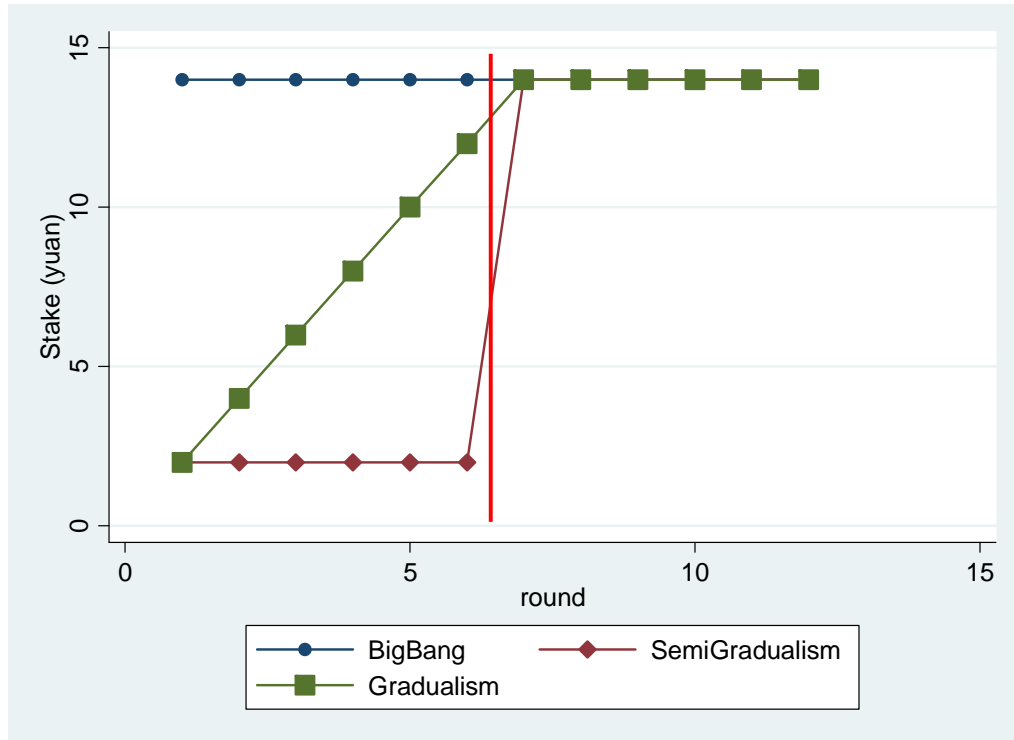
At the end of the experiments, I asked subjects to complete a brief survey. The survey collected information on age, gender, nationality, educational level, concentration at school, working status and income, in addition to eliciting risk preferences over lotteries.<sup>58</sup>

### **3.3.2. Treatment Group Assignments**

Each experiment consisted of three main treatments: “Big Bang,” “Semi-gradualism” and “Gradualism.” All groups in the three main treatments faced the same stake in the second half of the experiment (rounds 7-12); but the stake paths differed in the first half (rounds 1-6). The stakes over 12 rounds were shown in Figure 3.2: for the “Big Bang” treatment, the stakes were always at the highest level, which was 14; for the “Semi-gradualism” treatment, they were two for the first six rounds and set at the highest stake for the next six rounds; for the “Gradualism” treatment, they increased from two to 12 with a step of two for the first six rounds and fixed at the highest stake for the next six rounds.

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<sup>58</sup> The questions on risk preferences were adopted from Holt and Laury (2002), but in a different currency unit (see Appendix A.2).



The vertical line between rounds 6 and 7 separates the two halves of the experiment.

Figure 3.2: Stake Patterns of the Treatments

Since the stake paths for these treatments differed in rounds 1-6, it might have imposed an income effect in the laboratory. I adopted a standard way in the experimental literature to eliminate the income effect: randomly picking up one round, and using the earning of that round as the performance payment. Moreover, I randomized groups into various treatments, thus eliminating the potential effect of subjects' incomes in the real world.

Table 3.2 shows that randomizations worked very well in assigning subjects into the treatments. The default treatment is "Gradualism," so the constant shows the means for the "Gradualism" treatment, and the coefficients of "Big Bang" and "Semi-gradualism" show the differences between those two treatments and the "Gradualism" treatment. There is only one coefficient which is statistically significant from zero.

**Table 3.2: Comparison of Subjects' Characteristics by Treatment**  
Dependent Variable

	Age	Male	Income	Family Economic Status	Risk Aversion Index	Han Nationality	Student	Economics Major	Business Major
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Big Bang	0.550 (0.630)	-0.025 (0.113)	0.125 (0.239)	0.050 (0.198)	-0.275 (0.393)	0.025 (0.043)	0.025 (0.055)	0.025 (0.064)	0.050 (0.098)
Semi-gradualism	-0.200 (0.424)	-0.050 (0.113)	0.250 (0.247)	-0.450** (0.203)	-0.100 (0.414)	-0.075 (0.063)	0.050 (0.049)	0.000 (0.060)	-0.000 (0.095)
Constant	21.200***	0.525***	1.075***	2.575***	4.975***	0.950***	0.925***	0.075*	0.225***
Observations	120	120	120	120	120	120	120	120	120
R-squared	0.016	0.002	0.008	0.065	0.004	0.029	0.009	0.002	0.003

Note: The default treatment is "Gradualism," so the constant shows the means for the "Gradualism" treatment, and the coefficients of "Big Bang" and "Semi-gradualism" show the differences between those two treatments and the "Gradualism" treatment. Robust standard errors in parentheses. \* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%. For measures of variables, see notes in Table 3.1.



### 3.4. Results

In this section I analyze the effect of treatments on the following outcome variables per round: whether a trustor passes the stake, whether the trustee returns fairly conditional on the trustor sending, whether the group attains successful cooperation (i.e., whether the trustor sends and the trustee returns a fair amount), and the average earning of trustors, trustees and all subjects.

**Result 1: in round 1, the sending rate is high and similar for all three treatments.**

As shown in Figure 3.3, for all treatments, the sending rates are above 90%. Surprisingly, even the “Big Bang” treatment has such a high trust rate, which may be due to the repeated feature of the game: the trustor may want to signal his “trust” to induce reciprocation from the trustee, or she may want to screen whether the trustee is trustworthy. Of course, in the context of repeated interactions, the sending rate, as the ratio of amount sent to the trustor’s endowment in continuous trust games, does not measure pure trusting attitudes.

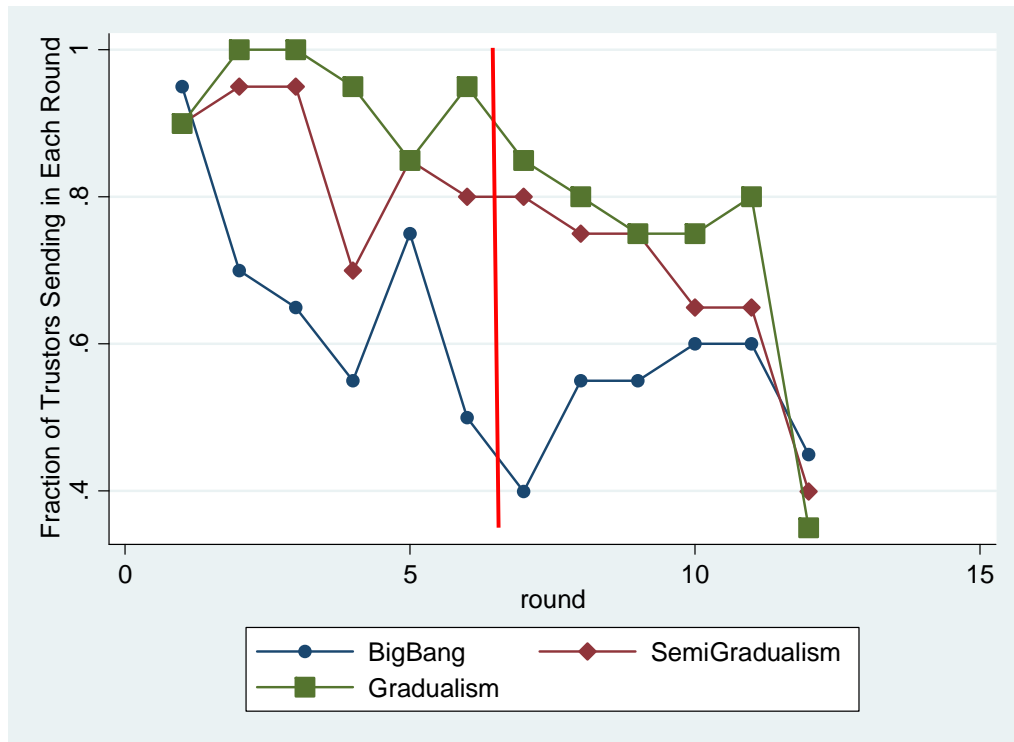
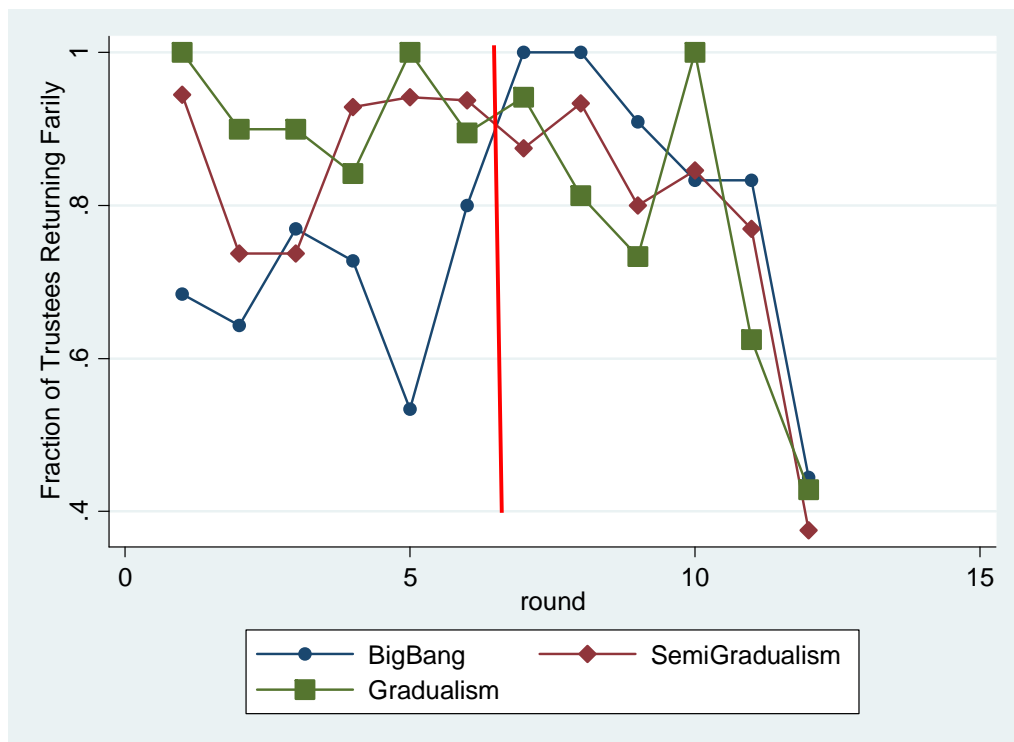


Figure 3.3: Fraction of Trustors Sending



Samples are restricted to the case when the trustor passes the stake.

Figure 3.4: Fraction of Trustees Returning Fairly Conditional on Trustor Sending

**Result 2: in round 1, conditional on trustors sending, the fraction of trustees returning a fair amount is lower for the “Big Bang” treatment; there is no difference between the “Semi-gradualism” and “Gradualism” treatments.**

The trustworthiness rate varies across treatments (see Figure 3.4). With a low stake, above 90% of trustees returns fairly in the first round; with a high stake (in the “Big Bang” approach), only below 70% of them do so. This difference is statistically significant. The reason may be that the benefit from betrayal is much higher when the trustee faces a high stake: if this round is selected for the final payment, the trustee has a chance to earn 59 *yuan* (including a show-up fee of 10 *yuan*) if he returns a low amount, which more than triples 17 *yuan*, the amount he will earn should he return a fair amount.

Figure 3.5 shows the rate of “successful cooperation” in each round. As mentioned above, cooperation of a group is defined as successful if both of the following conditions hold: the trustor sends the stake and the trustee returns a fair amount; otherwise it is considered as unsuccessful. So it is a product of trust and trustworthiness, and captures whether a group can achieve a Pareto outcome which has important meanings for certain policy aspects.

As a direct result of a high trust and a relatively lower trustworthiness in round 1, fewer groups in the “Bing Bang” treatment attain successful cooperation: about 65% of them are successful, versus a high rate above 85% for the other two treatments. There is no difference between the “Semi-gradualism” and “Gradualism” treatments.

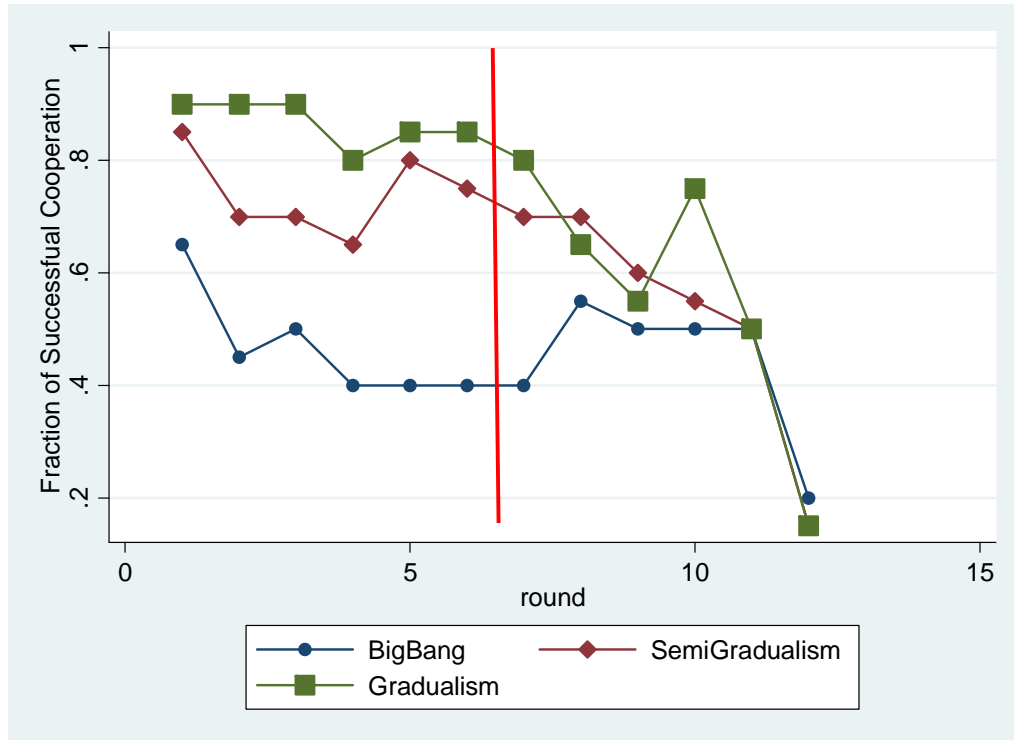


Figure 3.5: Fraction of Successful Cooperation (Trustor Sending and Trustee Returning Fairly)

**Result 3: from round 2, a gap appears between the sending rates of the “Big Bang” treatment and the other two treatments; there is no difference between the “Semi-gradualism” and “Gradualism” treatments.**

The sending rate of the “Bing Bang” treatment decreases sharply from about 95% to 70% in round 2, about 50% in round 6, and reaches a lowest of 40% in round 7, although it is temporally close to 80% in round 5. For the other two treatments, the sending rate fluctuates between 70% and 100%, but shows a general trend of slow decrease and reaches 95% and 80% in round 6 for “Gradualism” and “Semi-gradualism” treatments, respectively. Although the gap between a sudden increase of stake (the “Semi-gradualism” treatment) and a gradual increase (the “Gradualism” treatment) is insignificant in most rounds, that between

the “Big Bang” and “Gradualism” treatment remains until round 11, the second to the last round of the experiment, while that between the “Big Bang” and “Semi-gradualism” only remains until round 9.

Given the relatively lower rate of trustworthiness in round 1 in the “Big Bang” treatment, the sharp decrease of trust from round 1 to round 2 in this treatment is understandable. Table 3.3 presents a formal regression to capture the effect of trustees’ behavior in the previous round on the trustors’ in the current round. The sample restricts to the case when the trustors do send in the previous round. Specification 2 further restricts the sample to the case when the stakes do not change in two consecutive rounds. Both specifications show that trustors do respond positively and largely to trustees’ trustworthiness in the previous round. This parallels conditional cooperation behaviors in public good games (e.g., Fischbacher et al., 2001).

Table 3.3: The Effect of Early Trustworthiness on Trust

Dependent Variable: Trustor Sending		
	Whole Sample (1)	Restricted Sample (2)
Trustee Return Fairly in the previous round	0.456*** (0.0250)	0.499*** (0.0299)
Constant	0.556*** (0.0738)	0.554*** (0.0842)
Round Fixed Effects	Yes	Yes
Individual Fixed Effects	Yes	Yes
Observations	1,053	775
R-squared	0.550	0.606

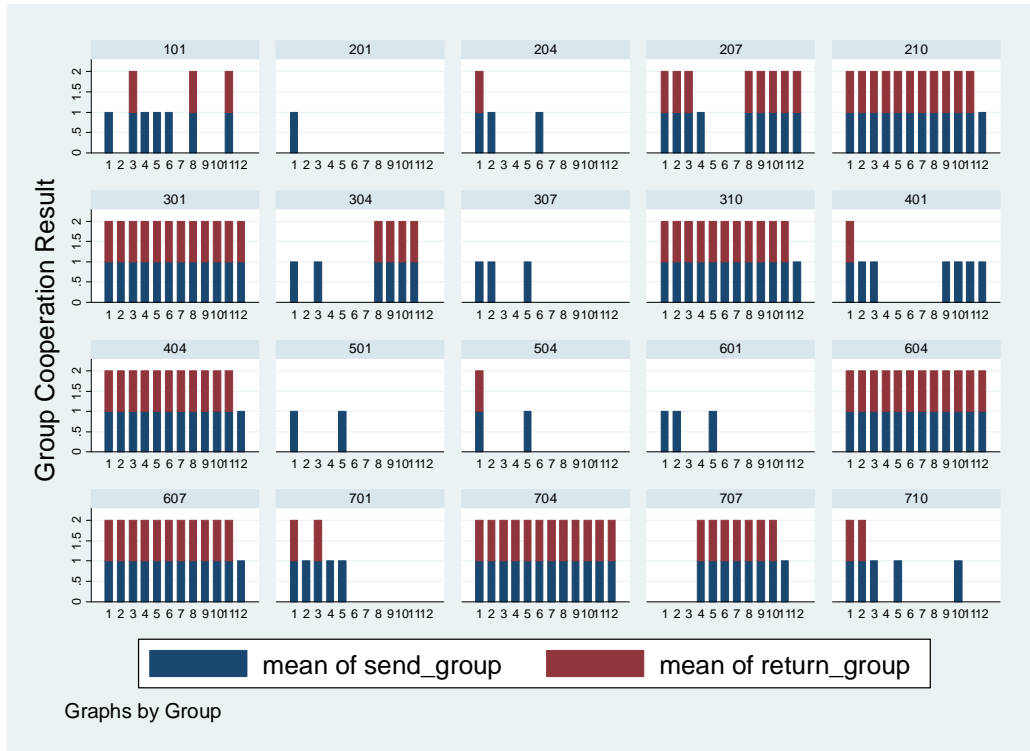
Note: Standard errors in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Probit regressions have qualitatively similar results. Specification 1 applies to all trustors in rounds 2-12 who trust in the previous round; specification 2 uses a subsample of specification 1 and only applies to the rounds when the stake does not change from the previous round, thus excluding round 7 for the “Semi-gradualism” treatment, and rounds 2-7 for the “Gradualism” treatment.

The result also suggests some kind of short-term tolerance in trustors’ response to trustees’ betrayal. The trustworthiness rate in the “Semi-gradualism” treatment decreases

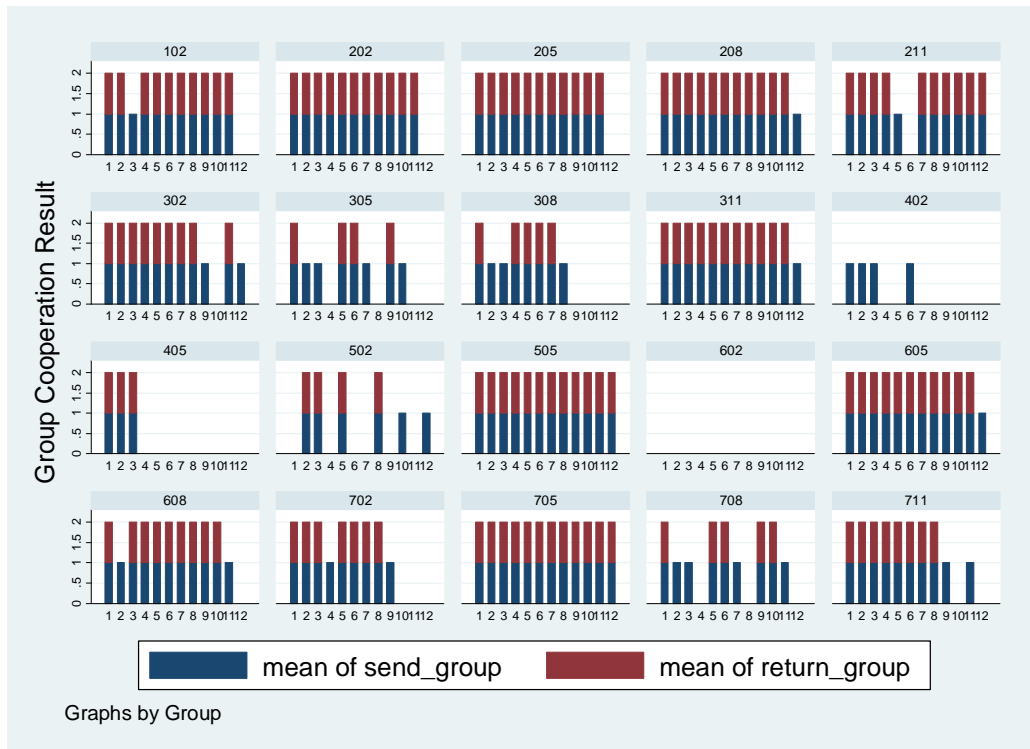
significantly in round 2 to slightly above 70% from a high 95% in round 1 and does not change from round 2 to round 3; but the trust rate for the “Semi-gradualism” treatment does not sharply decrease until round 4, which may show that the trustors tolerate the betrayal from trustees for one round (they could have stopped sending from round 3 should they become uncooperative), which parallels the finding by Fudenberg et al. (2011) in repeated prisoner’s dilemmas.

An obvious increase of trust rate is also found in round 5 for “Big Bang” and “Semi-gradualism” treatments. To examine the potential reason, Figure 3.6 shows cooperation results of each group by treatment. For each group, the horizontal axis indicates the round, and the vertical axis has two stacked bars in each round: the lower bar indicates whether the trustor sends, while the upper bar indicates whether the trustee returns fairly ; if the trustor does not send, both bars for the trustor and the trustee are empty (zero). On the top of the graph for each group is the 3- or 4-digit code for that group, which is constructed in the following way: the lowest two digits indicate the treatment type (1, 4, 7, 10=“Big Bang;” 2, 5, 8, 11=“Semi-gradualism;” 3, 6, 9, 12=“Gradualism”); the highest one digit indicates the session number (1-7).

According to Figure 3.6, we can see that the increase of trust rate for the “Big Bang” treatment in round 5 is that five trustors switch from no sending to sending, four (one) of which are betrayed (reciprocated) in an early round, stop trusting by round 4, but try investing again in round 5. Similar cases apply for the “Semi-gradualism” treatment.

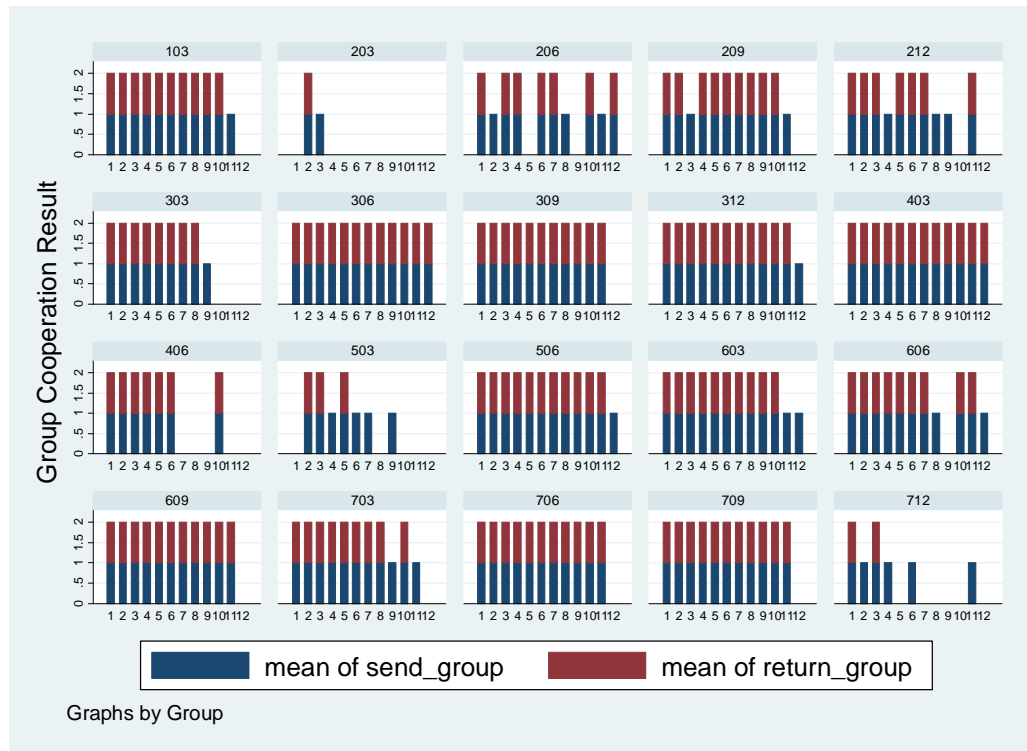


A: “Big Bang” Groups



B: “Semi-gradualism” Groups

Figure 3.6: Group Cooperation Results for Each Group in Each Treatment



C: “Gradualism” Groups

Figure 3.6 (Continued)

**Result 4: The gap between the trustworthiness rates of the “Big Bang” treatment and the other two treatments maintains for the first six rounds, but the gap disappears after that.**

Although we find a positive effect of a low beginning stake on trustworthiness in the first round, this effect is less persistent and the trustworthiness rate fluctuates (see Figure 3.4). This result is not so surprising because those less trustworthy may have been screened out over rounds by the trustors based on trustees’ actions, so those still receiving money from the trustors in later rounds may be more trustworthy in nature. In addition, given a trustor keeps trusting and sending a high stake, some trustees may become reciprocating



(even they may have betrayed in an early round); this suggests the coexistence of temptation to betray especially at a high stake and reciprocity to persistent trust.

For example, the trustworthiness rate of the “Semi-gradualism” increases from round 3 to round 4 and reaches 90% again. If we look at Panel A of Figure 3.6 for more details, we can find that it is a result of two factors: first, three betrayers in earlier rounds are no longer invested in (i.e., trusted) by trustors, thus those trusted in round 4 are those more trustworthy in nature (a screening effect); second, two trustees who betrayed in rounds 3 reciprocate the tolerance of the trustors (a reciprocation effect). The (conditional) trustworthiness rate of the “Big Bang” treatment increases in rounds 6 and 7, but it seems that it is all due to the screening effect: trustors betrayed in round 5 do not invest anymore. So the screening effect seems to dominate overall.

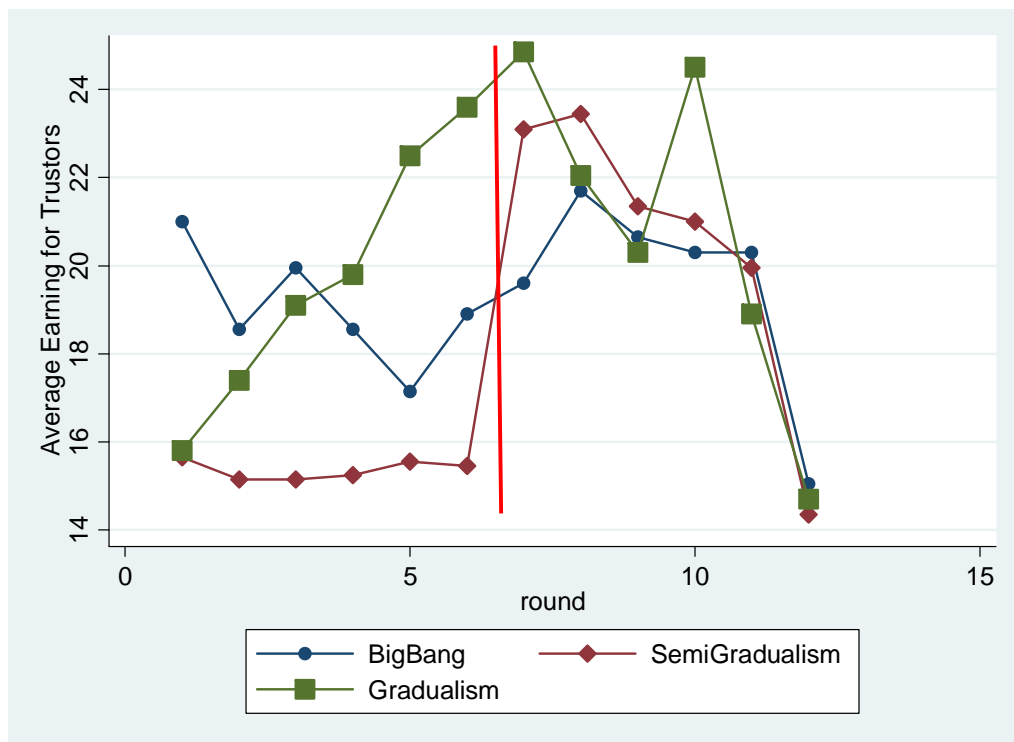
Due to the binary decision nature of the trustors, we cannot run a formal regression of trustworthiness on trust in the same round as conducted in the literature of continuous trust games. However, the ratio of trustees returning fairly (conditional on trustors sending the stake) is always higher than 50% for all treatments except for the end round, and is still about 40% in the end round, which does suggest some reciprocity.<sup>59</sup> Moreover, as shown above, there are suggestive evidences that some trustees who have betrayed earlier start rewarding trustors if the trustors tolerate their betrayals and keep investing in trustees.

As a consequence of results 3 and 4, the gap between the rate of successful cooperation of the “Big Bang” treatment and the other two treatments maintains for the first seven rounds, but the gap is unobvious after that.

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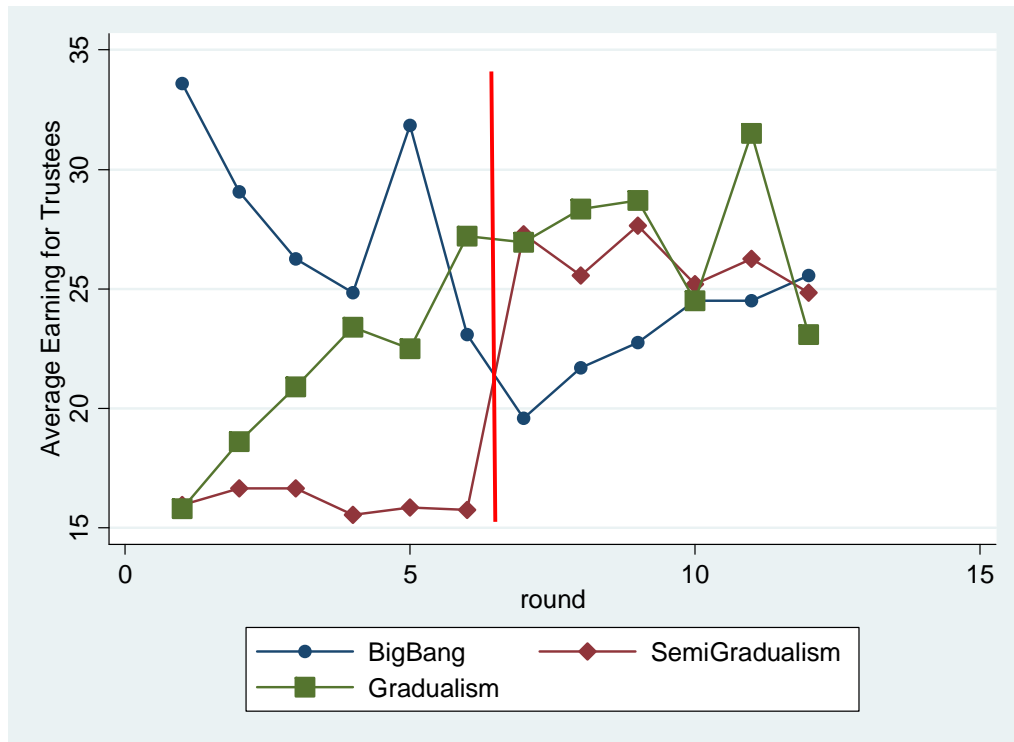
<sup>59</sup> Of course, this may not reflect pure reciprocity and may involve fairness concern or altruism (Cox, 2004).

Figures 7-9 explore earnings of trustors, trustees, and all subjects in each round for all three treatments. It is not surprising that the “Big Bang” treatment has the highest average earning at the beginning for trustors, trustees and both, since it has a higher earning potential with a high stake and the rates of trust and trustworthiness are all above 70%. However, this advantage of “Big Bang” treatment disappears after 4-5 rounds when cooperation falls apart, and the “Gradualism” treatment becomes dominant for several rounds until near the end (rounds 10-12). The “Semi-gradualism” treatment has lowest earnings for the first half (rounds 1-6) due to the low stake (low earning potential), but there is no significant difference between it and the “Gradualism” treatment after that when they both face the same stake.

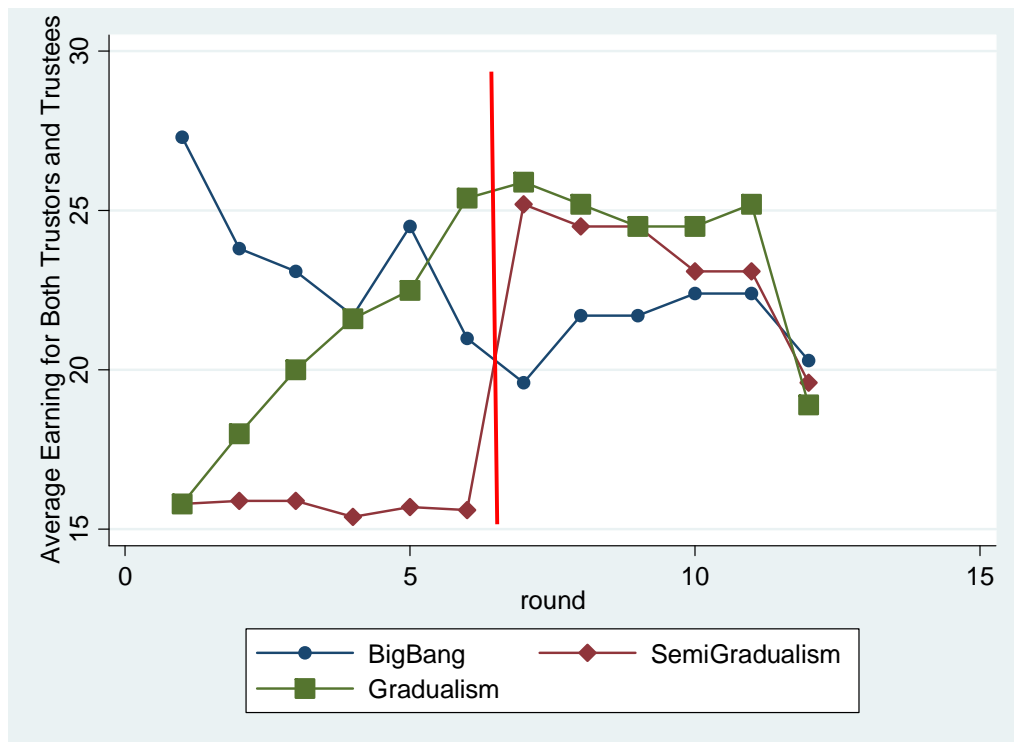


The endowment (14 *yuan*) is included in the earning.

Figure 3.7: Average Earning for Trustors



The endowment (14 *yuan*) is included in the earning.  
Figure 3.8: Average Earning for Trustees



The endowment (14 *yuan*) is included in the earning.  
Figure 3.9: Average Earning for Both Trustors and Trustees

The results regarding individual earnings lead to mixed policy implications. The “Big Bang” approach may not be bad for social welfare if the time horizon is relatively short (as long as it is long enough for the selfish trustees to reciprocate in the beginning interactions), but starting with a low stake may lead to higher social welfare if there are many high-stake investment opportunities with the same partner: starting with a low stake helps remain high trust and trustworthiness except in the end rounds, which yields more profits for both parties when the stake becomes high.

Table 3.4 presents the differences among different treatments regarding the rates of trust, trustworthiness and successful cooperation in rounds 7-12 with the high stake. It employs a regression form: the default treatment is “Gradualism,” so the constant coefficient shows the means for the “Gradualism” treatment, and the coefficients of “Big Bang” and “Semi-gradualism” show the differences between those two treatments and the “Gradualism” treatment. It clearly shows that starting at a low stake leads to a higher trust rate (except the end round) in rounds 7-12, but not a higher trustworthiness rate, and a higher rate of successful cooperation only in round 7. No difference between a gradual increase (the “Gradualism” treatment) in stake and a sudden increase (the “Semi-gradualism” treatment) is detected.

**Table 3.4: Sending, Returning Fairly and Successful Cooperation in rounds 7-12 by Treatments**

	Trustor Sending			Trustee Returning Fairly			Successful Cooperation		
	Period 7	Period 12	Period 7-12	Period 7	Period 12	Period 7-12	Period 7	Period 12	Period 7-12
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Big Bang	-0.450*** (0.098)	0.100 (0.096)	-0.192** (0.074)	0.059 (0.063)	0.016 (0.288)	0.051 (0.092)	-0.400*** (0.073)	0.050 (0.090)	-0.125** (0.051)
Semi-gradualism	-0.050 (0.126)	0.050 (0.119)	-0.050 (0.113)	-0.066 (0.098)	-0.056 (0.237)	0.010 (0.105)	-0.100 (0.105)	0.000 (0.110)	-0.033 (0.149)
Constant	0.850*** (0.067)	0.350** (0.105)	0.717*** (0.052)	0.941*** (0.063)	0.429* (0.207)	0.791*** (0.046)	0.800*** (0.075)	0.150* (0.067)	0.567*** (0.062)
Observations	60	60	360	41	24	229	60	60	360
R-squared	0.187	0.007	0.028	0.032	0.004	0.003	0.124	0.004	0.011

Note: The default treatment is "Gradualism," so the constant shows the means for the "Gradualism" treatment, and the coefficients of "Big Bang" and "Semi-gradualism" show the differences between those two treatments and the "Gradualism" treatment. Robust standard errors in parentheses. \* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%. All variables are binary. Standard errors are clustered at session level. There are 120 subjects in 60 groups of 7 sessions.

**Result 5: There is an end-of-game effect: in the last round the rates of trust, trustworthiness and successful cooperation fall sharply for all three treatments; there is no performance difference among these three treatments.**

This shows that although starting at the low stake helps achieve trust at a high stake, it cannot solve the end-of-game effect in which both trust and trustworthiness fall apart. This is partly consistent with the standard economic theory and the similar effect found in finitely-repeated continuous public good experiments, as well as other studies on finitely-repeated trust game (Cochard et al., 2004; Engle-warnick & Slonim, 2004) and market (buyer-seller) game (Bolton et al., 2004).<sup>60</sup> However, the rates of trust and trustworthiness in the last round are both about 40%, which is far from zero predicted by the standard theory.

### **3.5. Conclusion and Discussion**

The findings in this chapter show that gradualism -- defined as increasing the stake level of investment step by step -- outperforms the *big bang* approach to a certain extent: although the initial trust is high no matter how large the stake is, starting at a low stake leads to a higher initial trustworthiness and thus higher trust in subsequent interactions. I do not detect the difference in trust levels between a sudden increase in stake and a gradual increase. Unfortunately, trustworthiness and trust for all treatments sharply decrease in the end, suggesting gradualism has its limitations in finite repetitions. Future studies are desired to examine whether gradualism outperforms the *big bang* approach in all high-stake rounds when repetitions are infinite, or whether combining gradualism with perfect monitoring,

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<sup>60</sup> Engle-warnick and Slonim (2004) compares finitely-repeated and infinite-repeated trust games, and find that the level of trust is indistinguishable between these two when subjects are inexperienced. However, as subjects gain experience, the level of trust decreases in the finite game but does not change in the infinite game.

social pressure, sanctions or other institutions helps it avoid the end-of-game effect in finite plays.

In this experiment in each round the trustor can only decide to send a certain predetermined amount (the stake) or not, rather than send any amount freely. This reflects many cases in the real world when the trustor decides whether to invest in a project with a certain stake. The binary choice nature with an exogenous stake, which is known to both trustors and trustees, may have helped overcome the dilemma faced by the trustors (“first movers”): if they do not send all or almost all of what they can, the trustees may not feel really trusted and thus not reciprocate to reward the trustors; but if they send all, they expose themselves to a huge risk (Pillutla et al., 2003). Comparing the exogenously gradual (and binary) approach in this experiment with the method of completely free choices deserves future experimental explorations.

This chapter only studies finitely-repeated trust games. It would be also interesting to compare finite plays with one-shot games as well as infinite plays. I leave this for future studies.

## **APPENDIX**



## **A.1 Experimental Instructions for Coordination Experiments 1-3<sup>61</sup>**

The study is conducted anonymously. Subjects will be identified only by code numbers. There is no communication among the subjects. The experiment will last from 30 minutes to one hour. Please raise your hand if anything is unclear to you.

### **Experiment Structure**

This experiment will consist of two independent stages. You will receive instructions for each stage on the screen before that stage begins. [For experiments 2 and 3: You will be told the exact number of periods of each stage when you enter that stage.] In each stage, you are playing in a group of 4 members (including yourself). For each stage, the group members are randomly selected and would NOT change during that stage. However, the groups would be reshuffled in new groups after the first stage.

### **Rule of Each Period**

Please note that the experiment consists of two stages, and each stage has some periods. The following rule applies to each period.

In each period, you are assigned an endowment of 20 points, and asked to give a stated amount to a group pool. The stated amount may (or may not) change across periods. In each period you can decide whether not to give, or to give exactly that amount, but can not give other points. [For experiments 2 and 3: At the beginning of each stage, you will be told the

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<sup>61</sup> Coordination Experiments 1-3 refer to the weakest-link experiment with limited information feedback, the weakest-link experiment with richer information feedback, and the non-weakest-link experiment, respectively. Chapter 1 discusses only coordination experiment 1, while Chapter 2 discusses all three. Original instructions are in Chinese. Contents in square brackets are only shown to subjects of the specific experiment(s) (“For experiment(s) X” is not shown). Subjects in one experiment are not aware of other experiments.

values of these stated amounts of all periods in that stage.] You cannot know others' choices when you make your own decision. [For experiment 1: After each period, you will know whether all your group members (including yourself) give the stated amount, but if not all of you give, you will not know how many members give.] [For experiments 2 and 3: After each period, you will know how many members in your group (including yourself) give the stated amount.]

[For experiments 1 and 2:

If all 4 members of your group (including yourself) give the stated amount, you will get twice that amount back (thus having a net return equaling that amount). But if not all of your group members give, you will NOT get any of your given points back and will thus end the period with only the points you do not give.

So in each period, your earning will depend on the following cases:

Case 1: If all 4 members give the stated amount, then you earn:  $20 + \text{that amount}$

Case 2: If you give, but not all other 3 members give, then you earn:  $20 - \text{that amount}$

Case 3: If you do not give, no matter whether other members give or not, then you earn:

20

A special case of Case 3 is as follows:

- Case 4: If all 4 members do not give, then you earn 20 (each of 4 members earns 20)

]

[For experiment 3:

If at least three members of your group (including yourself) give the stated amount, you will get twice that amount back (no matter whether you give that amount or not); if all 4 members give, the extra amount will be equally divided for all 4 members. But if fewer than three members give, you will NOT get any of your given points back and will thus end the period with only the points you do not give.

The table below lists your earning in each period for different cases. X indicates the stated amount each one is asked to give.

<b>Your Choice</b>	<b>The Number of Other Group Members Who Give</b>	<b>Your earning</b>
Give	3	$20 - X + 2X + X/4 = 20 + 1.25X$
Give	2	$20 - X + 2X = 20 + X$
Give	$<2$	$20 - X$
Not Give	3	$20 + 2X$
Not Give	$<3$	20

]

### Examples

[For experiments 1 and 2:

Example 1: You are asked to give 10 points. You give, and all other group members also give. Then in this period each of you earns  $20 + 10 = 30$  points.

Example 2: You are asked to give 10 points. You give, two other group members also give, but the last one does not. Then in this period, each of you and the other two members earns  $20 - 10 = 10$  points, while the last member earns 20 points.

Example 3: You are asked to give 10 points. You do not give, but all other group members give. Then in this period you earn 20 points, and each of the three other members earns  $20 - 10 = 10$  points.

]

[For experiment 3:

Example 1: You are asked to give 10 points. You give, and all other group members also give. Then in this period each of you earns  $20+1.25*10=32.5$  points.

Example 2: You are asked to give 10 points. You give, two other group members also give, but the last one does not. Then in this period, each of you and the other two members earns  $20+10=30$  points, while the last member earns  $20+2*10=40$  points.

Example 3: You are asked to give 10 points. You give, but only one of the other three group members gives. Then in this period, each of you and the other one who gives earns  $20-10=10$  points, while the other two member earns 20 points each.

Example 4: You are asked to give 10 points. You do not give, but all other three group members give. Then in this period you earn  $20+2*10=40$  points, and each of the other three members earns  $20+10=30$  points.

Example 5: You are asked to give 10 points. You do not give, and only two of the other three group members give. Then in this period each of you and the other one who does not give earns 20 points, and each of the two members who give earns  $20-10=10$  points.

]

We will have examinations on the computer to make sure you understand the rule. You can start the experiment only after you answer all questions correctly.

### **Payment**

[For experiment 1: Your final payment for this experiment is the sum of two parts. The first is a show-up fee of about 400 points.<sup>62</sup> The second is a performance payment, i.e., the sum of your earnings from all periods in two stages. The conversion rate is 40 points = ¥1.00. All payments will be in cash. ]

[For experiments 2 and 3: Your final payment for this experiment is the sum of three parts. The first is your earning from the first stage: at the end of the experiment, the computer will randomly pick up one period from the first stage and shows it on your computer screen (this period will apply to all subjects in this session); your earning in the first stage is that in the chosen period times the number of periods in the first stage, while your earning in other periods of the first stage will be discarded. The second is the sum of your earnings from all periods in the second stage. The third is a show-up fee of ¥10. The conversion rate is 30 points = ¥1.00. All payments will be in cash. ]

At the end of the experiment, you will be asked to fill out a simple questionnaire. Then you can collect your earnings by presenting your code number to the supervisor. Your earnings will be in an envelope marked with your code number.

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<sup>62</sup> For sessions without the “High Show-up Fee” treatment, it is stated as “a show-up fee of 400 points.”

## A.2 Risk Aversion Questions

Your code\_\_\_\_\_

In the table below, you are presented with a choice between two lotteries, A or B, along with the payoff matrix for each lottery.

For example, the first row shows that lottery A offers a 10% chance of receiving ¥20.00 and a 90% chance of receiving ¥16.00. Similarly, lottery B offers a 10% chance of receiving ¥38.50 and a 90% chance of ¥1.00.

In the third table column, simply indicate given the two lotteries in each row, which one would you prefer if you are given the choice? A or B for each row?

Lottery A				Lottery B				Your lottery choice
prob(¥20.00)		prob(¥16.00)		prob(¥38.50)		prob(¥1.00)		
0.1	¥20.00	0.9	¥16.00	0.1	¥38.50	0.9	¥1.00	
0.2	¥20.00	0.8	¥16.00	0.2	¥38.50	0.8	¥1.00	
0.3	¥20.00	0.7	¥16.00	0.3	¥38.50	0.7	¥1.00	
0.4	¥20.00	0.6	¥16.00	0.4	¥38.50	0.6	¥1.00	
0.5	¥20.00	0.5	¥16.00	0.5	¥38.50	0.5	¥1.00	
0.6	¥20.00	0.4	¥16.00	0.6	¥38.50	0.4	¥1.00	
0.7	¥20.00	0.3	¥16.00	0.7	¥38.50	0.3	¥1.00	
0.8	¥20.00	0.2	¥16.00	0.8	¥38.50	0.2	¥1.00	
0.9	¥20.00	0.1	¥16.00	0.9	¥38.50	0.1	¥1.00	
1	¥20.00	0	¥16.00	1	¥38.50	0	¥1.00	

### A.3 Proof of Lemma 2.1

**Lemma 2.1.**  $0 < \theta_i(Th_i) < 1$ , and  $\partial \theta_i(Th_i) / \partial Th_i > 0$ .

**Proof :** Let  $\theta_i$  indicates the minimum probability of success which makes  $i$  willing to contribute, and  $\beta = \alpha - 1 > 0$ . Then  $U_i(E) = \theta_i U_i(E + \beta Th_i) + (1 - \theta_i) U_i(E - Th_i)$ . So,

$$\theta_i = \frac{U_i(E) - U_i(E - Th_i)}{U_i(E + \beta Th_i) - U_i(E - Th_i)}$$

$$U_i'(x) > 0 \Rightarrow U_i(E + \beta Th_i) - U_i(E - Th_i) > U_i(E) - U_i(E - Th_i) > 0 \Rightarrow 0 < \theta_i < 1$$

$$\partial \theta_i / \partial Th_i$$

$$= \frac{U_i'(E - Th_i)[U_i(E + \beta Th_i) - U_i(E - Th_i)] - [U_i(E) - U_i(E - Th_i)][\beta U_i'(E + \beta Th_i) + U_i'(E - Th_i)]}{[U_i(E + \beta Th_i) - U_i(E - Th_i)]^2}$$

$$\equiv \frac{A}{B}$$

Since  $B > 0$ , to prove  $\partial \theta_i(Th_i) / \partial Th_i > 0$ , I just need to show  $A > 0$ .

$$\begin{aligned} A &= U_i'(E - Th_i)U_i(E + \beta Th_i) - U_i'(E - Th_i)U_i(E - Th_i) - U_i(E)\beta U_i'(E + \beta Th_i) \\ &\quad + U_i(E - Th_i)\beta U_i'(E + \beta Th_i) - U_i(E)U_i'(E - Th_i) + U_i(E - Th_i)U_i'(E - Th_i) \\ &= U_i'(E - Th_i)[U_i(E + \beta Th_i) - U_i(E)] - \beta U_i'(E + \beta Th_i)[U_i(E) - U_i(E - Th_i)] \end{aligned}$$

$$A > 0 \Leftrightarrow \frac{U_i(E + \beta Th_i) - U_i(E)}{\beta U_i'(E + \beta Th_i)} > \frac{U_i(E) - U_i(E - Th_i)}{U_i'(E - Th_i)}$$

$$\Leftrightarrow \frac{\int_E^{E+\beta Th_i} U_i'(x) dx}{\beta U_i'(E + \beta Th_i)} > \frac{\int_{E-Th_i}^E U_i'(x) dx}{U_i'(E - Th_i)} \quad (A3.1)$$

$U_i''(x) < 0 \Rightarrow U_i'(x) > U_i'(E + \beta Th_i)$  for all  $x \in [E, E + \beta Th_i]$ , and

$U_i'(x) < U_i'(E - Th_i)$  for all  $x \in (E - Th_i, E]$ .

$$\text{LHS of (A3.1)} > \frac{\int_E^{E+\beta Th_i} U_i'(E + \beta Th_i) dx}{\beta U_i'(E + \beta Th_i)} = \frac{\beta Th_i U_i'(E + \beta Th_i)}{\beta U_i'(E + \beta Th_i)} = Th_i$$

$$\text{RHS of (A3.1)} < \frac{\int_{E-Th_i}^E U_i'(E - Th_i) dx}{U_i'(E - Th_i)} = \frac{Th_i U_i'(E - Th_i)}{U_i'(E - Th_i)} = Th_i$$

So  $A > 0$ ,  $\partial \theta_i(Th_i) / \partial Th_i > 0$ . *Q.E.D.*

## A.4 Proof of Proposition 2.1

**Proposition 2.1.** The lower the  $Th_1$ , the higher the probability that the coordination at  $t=1$  will succeed.

**Proof:** since  $\partial s_{j,1}^i / \partial Th_1 = -\partial F_{j,1}^i(Th_1) / \partial Th_1 \leq 0$ ,  $1-\varepsilon-\eta > 0$ ,  $(1-\varepsilon)s_{j,t}^i + \eta(1-s_{j,t}^i) \geq 0$ , then

$\partial \prod_{j \neq i} [(1-\varepsilon)s_{j,t}^i + \eta(1-s_{j,t}^i)] / \partial Th_1 \leq 0$ . But  $\partial \theta_i(Th_1) / \partial Th_1 \geq 0$ . According to (2.1), the lower

the  $Th_1$ , the higher the probability the LHS of (2.1) will be larger than the RHS, i.e., the higher the probability that a level-1 player  $i$  will contribute at  $t=1$ .

For a level-2 player  $k$ , she will contribute if and only if she believes that the probability that all her opponents (in her mind all her opponents are level-1 players) will give is larger than or equal to  $\theta_k(Th_1)$ , her “reserve probability of success.” Since a level-2 player knows the strategy rule of level-1 players, she believes (correctly expects) that the lower the  $Th_1$ , the higher the probability that a level-1 player  $i$  will contribute at  $t=1$ . So the lower the  $Th_1$ , the higher the probability that a level-2 player  $k$  will contribute at  $t=1$ .<sup>63</sup>

For a level-0 player  $j$ , the probability that she will give is  $(1-\varepsilon)1(B_j \geq Th_1) + \eta 1(B_j < Th_1)$ , where  $1(\bullet)$  equals one if the argument in the parenthesis is true, and zero otherwise, and  $B_j$  is her willingness-to-give. Given  $(1-\varepsilon) > \eta$ , it is obvious that the lower the  $Th_1$ , the higher the probability that a level-0 player  $j$  will contribute at  $t=1$ .

As shown above, the lower the  $Th_1$ , the higher the probability that any level-0, level-1 and level-2 player will contribute at  $t=1$ , so the higher the probability that the coordination at  $t=1$  will succeed. *Q.E.D.*

---

<sup>63</sup> Due to the similar reason, the lower the  $Th_1$ , the higher the probability that a level-3, level-4 player will contribute at  $t=1$ , etc. So Proposition 2.1 holds for general level- $k$  thinking.



## **A.5      Supplementary Sessions of Limited-information Weakest-link Experiments**

To further check whether the difference of the gradualism effect in coordination experiments 1 and 2 in Chapter 2 (the two weakest-link experiments) is driven by the information about the exact number of contributors, I ran several further sessions (five groups for each treatment) of weakest-link experiments with limited information. The design of these sessions is identical to experiment 2 except that subjects are told whether coordination is successful but not the exact number of group members contributing (if less than four contribute), and they were conducted in the same laboratory as experiment 2 in July 2011. This is to confirm that the different effects of gradualism between experiments 1 and 2 are not due to other factors (e.g., location, payment method, conversion rate, and information about the number of periods and stake trend).

The results are shown in Figures A.1 – A.3 and summarized as below. First, success rates are lower in these sessions than in experiment 2 (it is also lower than experiment 1.) Second, the consistency of success/failure over periods is more obvious than in experiment 2. Third, no failed “Big Bang” groups can become successful.

Although the sample size of these sessions is small, it is clear that their results are more similar with experiment 1 rather than experiment 2. So the difference between experiments 1 and 2 is mostly due to the information regarding the exact number of contributors.

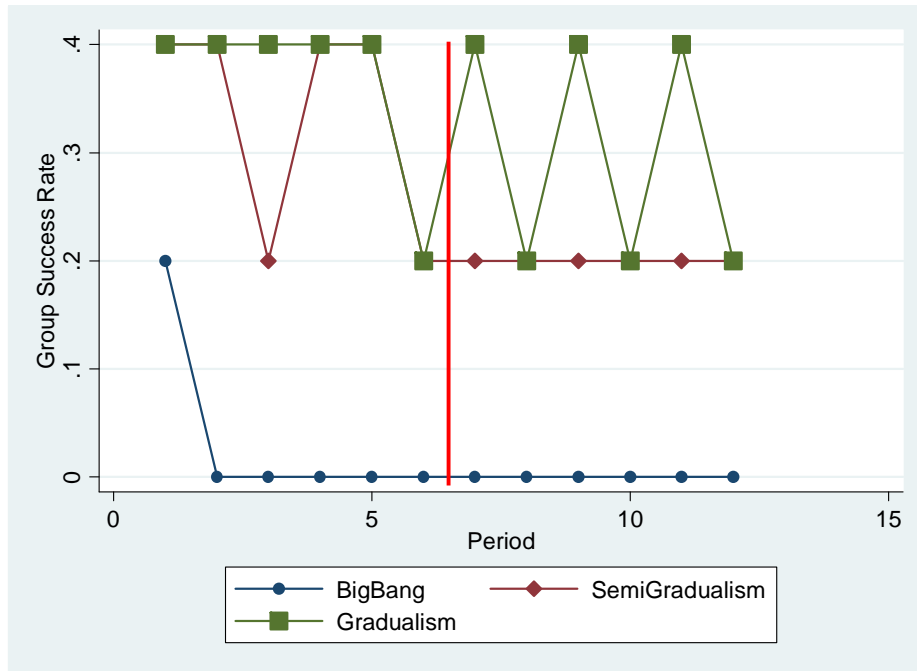


Figure A.1 Success Rates of Groups by Treatment and Period for Supplementary Sessions

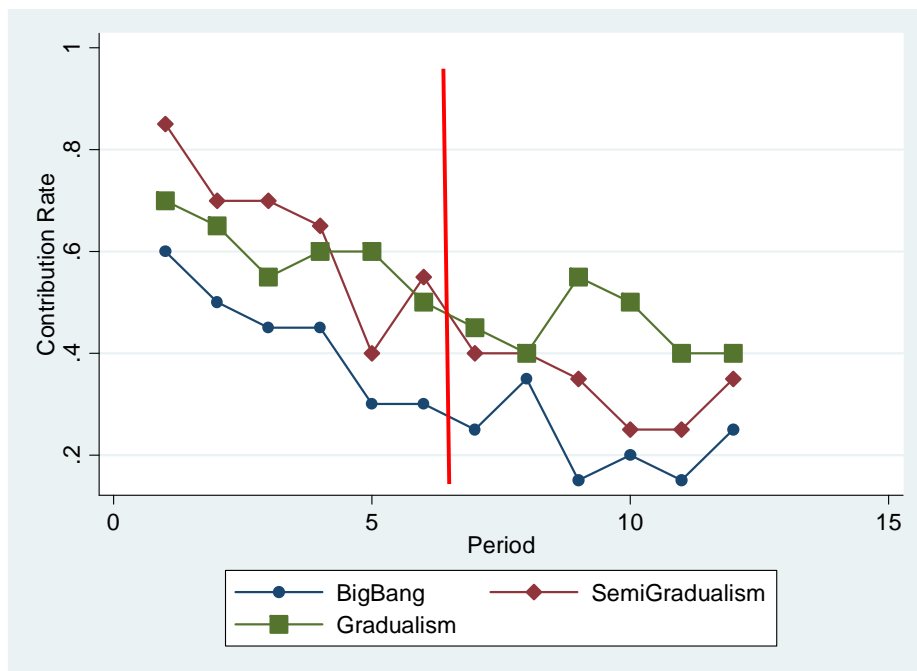


Figure A.2 Contribution Rates by Treatment and Period for Supplementary Sessions

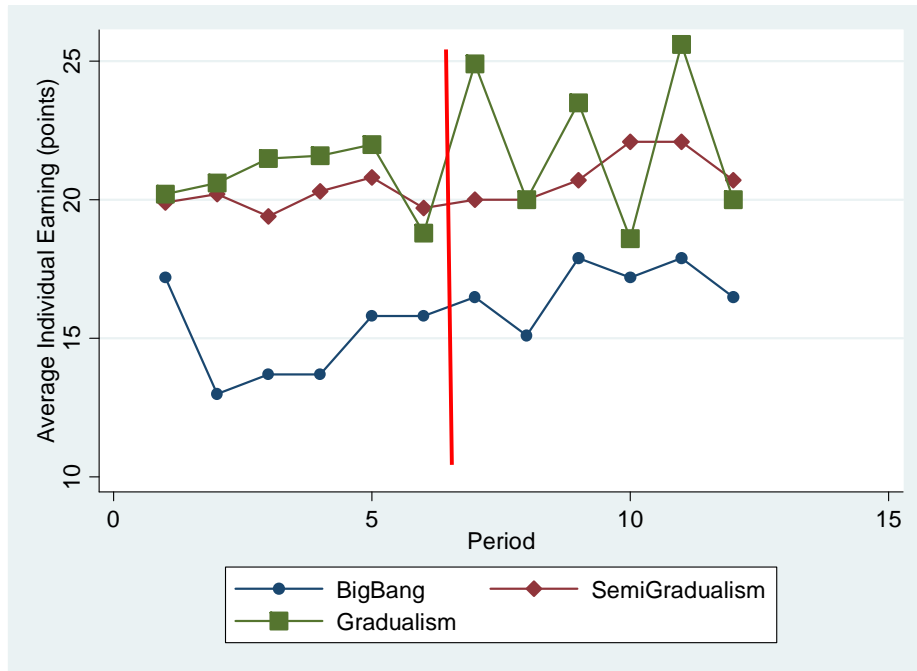


Figure A.3 Individual Earnings by Treatment and Period for Supplementary Sessions

## **A.6: Experimental Instruction for the Trust Experiment in Chapter 3**

The study is conducted anonymously. Subjects will be identified only by code numbers. There is no communication among the subjects. The experiment will last from 30 minutes to one hour. Please raise your hand if anything is unclear to you.

### **Experiment Structure**

There will be two roles in this experiment: “Person A” and “Person B.” Each of you only plays one of these two roles during the whole experiment. Your role will be determined randomly by the computer at the beginning of the experiment.

The experiment includes 12 rounds. You will be randomly matched with another person who plays the opposite role: if you play role A, the other will play role B; if you play role B, the other will play role A. You will play with the same person during the whole experiment.

### **Rule of Each Period**

Please note that the experiment has 12 rounds. The following rule applies to each round.

In each round, each of you receives an endowment of 14 *yuan*. Then “Person A” will be asked to pass  $X$  *yuan* to “Person B.” This number ( $X$ ) may (or may not) vary over rounds and can be any even number between 0 and 14 (including 0 and 14), and it is known by both “Person A” and “Person B.” The path of  $X$  in all rounds depends on the group you are in, and will be told to you when the experiment starts.

“Person A” first decides whether to pass  $X$  yuan or to pass nothing to “Person B.” That is to say, the amount of money that “Person A” passes should be either 0 or  $X$  yuan, not any other number. If “Person A” chooses NOT to pass  $X$  yuan, then each of “Person A” and “Person B” ends the round with 14 yuan. Otherwise the  $X$  yuan sent out will be tripled to be  $3X$ . “Person B” knows the decision of “Person A” after “Person A” makes a decision. Then “Person B” decides how many points to return to “Person A.” “Person B” also has two choices: either to return  $2X$  yuan, or to return  $0.5X$  yuan. If “Person B” chooses to return  $2X$  yuan, “Person A” ends the round with  $14-X+2X=14+X$  yuan, and “Person B” ends the round with  $14+3X-2X=14+X$  yuan, respectively; if “Person B” chooses to return  $0.5X$ , “Person A” ends the round with  $14-X+0.5X=14-0.5X$  yuan, and “Person B” ends the round with  $14+3X-0.5X=14+2.5X$  yuan, respectively.

The above rule of each round is shown in the table below.

Person A's choice	Person B's choice	Person A's earning for this round	Person B's earning for this round
Pass 0		14	14
Pass $X$	Return $2X$	$14+X$	$14+X$
	Return $0.5X$	$14-0.5X$	$14+2.5X$

In each round, the actions of “Person A” and “Person B” are known to each other after each action.

### Examples

Example 1: “Person A” is asked to pass 10 yuan to “Person B.” “Person A” passes 10 yuan, so the 10 yuan triple to be 30 yuan. Then “Person B” returns  $2*10=20$  yuan. In this round “Person A” earns  $14-10+20=24$  yuan, while “Person B” earns  $14+30-20=24$  yuan.

Example 2: “Person A” is asked to pass 10 *yuan* to “Person B.” “Person A” passes 10 *yuan*, so the 10 *yuan* triple to be 30 *yuan*. Then “Person B” returns  $0.5 \times 10 = 5$  *yuan*. In this round “Person A” earns  $14 - 10 + 5 = 9$  *yuan*, while “Person B” earns  $14 + 30 - 5 = 39$  *yuan*.

Example 3: “Person A” is asked to pass 10 *yuan* to “Person B.” “Person A” does not pass. In this round each of “Person A” and “Person B” earns 14 *yuan*.

We will have examinations on the computer to make sure you understand the rule. You can start the experiment only after you answer all questions correctly.

## **Payment**

Your final payment for this experiment is the sum of two parts. The first is your performance payment: at the end of the experiment, the computer will randomly pick up one round from the 12 rounds and shows it on your computer screen (this round will apply to all subjects in this session); your performance payment is your earning in the chosen round, while your earning in other rounds will be discarded. The second is a show-up fee of ¥10. All payments will be in cash.

At the end of the experiment, you will be asked to fill out a simple questionnaire. Then you can collect your earnings by presenting your code number to the supervisor. Your earnings will be in an envelope marked with your code number.

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